



# THE UNIVERSITY *of* EDINBURGH

This thesis has been submitted in fulfilment of the requirements for a postgraduate degree (e.g. PhD, MPhil, DClinPsychol) at the University of Edinburgh. Please note the following terms and conditions of use:

- This work is protected by copyright and other intellectual property rights, which are retained by the thesis author, unless otherwise stated.
- A copy can be downloaded for personal non-commercial research or study, without prior permission or charge.
- This thesis cannot be reproduced or quoted extensively from without first obtaining permission in writing from the author.
- The content must not be changed in any way or sold commercially in any format or medium without the formal permission of the author.
- When referring to this work, full bibliographic details including the author, title, awarding institution and date of the thesis must be given.

SCIENTIFIC NATURALISM IN VICTORIAN BRITAIN:

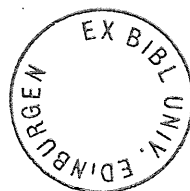
An Essay in the Social History of Ideas.

L. S. Jacyna

Ph.D

University of Edinburgh

1980



## ABSTRACT

This thesis considers, from a sociological viewpoint, the intellectual movement in Victorian Britain known as scientific naturalism. It argues that the naturalist cosmology needs to be seen as part of the strategy of certain social groups; in particular, naturalism expressed the interests of the newly-emerging scientific profession in nineteenth century Britain. The professionalisation of science was part of a larger social development: the appearance of a 'new' professional middle-class. The thesis considers how other new professionals, especially those connected with medicine, deployed naturalistic formulations in their own attempts to secure social recognition and resources. An attempt is made to place naturalism in a broader historical perspective as well as to describe the intellectual background from which it emerged.

There are six chapters. The first describes social conditions relevant to an understanding of naturalism; the next four discuss the leading themes of the naturalist world-view; the last considers the wider significance of naturalistic approaches to man and society at the turn of the nineteenth century.

## ACKNOWLEDGEMENTS

I wish to thank Donald MacKenzie who supervised the writing of this thesis and whose own researches provided a starting-point and exemplar. I am grateful to Barry Barnes for his continued interest in my work, and to Steven Shapin for his comments on the first draft.

My research was funded by the Social Science Research Council.

Some of the argument of Chapter Six appeared as Jacyna 1980.



DLA MATKI

## TABLE OF CONTENTS

<b>Introduction .....</b>	<b>1</b>
<b>Chapter One: The Context of Naturalism .....</b>	<b>14</b>
i. The Strategy of Naturalism.....	14
ii. The Bifurcation of Science and Theology .....	19
iii. The Struggle for Resources .....	26
iv. Naturalism and Enlightenment .....	42
<b>Chapter Two: Causes, and Forces, and Powers .....</b>	<b>51</b>
i. Spiritualists, Materialists, and Sceptics: the Eighteenth Century Background	51
ii. Nineteenth Century Newtonianism .....	56
iii. Theological Responses to the Naturalist Programme .....	67
iv. The Naturalism of Substance .....	70
v. The Naturalism of Method .....	80
vi. The Sovereignty of God and the Freedom of Man .....	92
<b>Chapter Three: The Physical Basis of Life .....</b>	<b>103</b>
Introduction .....	103
i. Old Vitalism .....	104
ii. The Fabric of Life .....	117
iii. The Stuff of Life .....	131
iv. "New" Vitalism .....	149
<b>Chapter Four: Mind and Nature .....</b>	<b>160</b>
Introduction .....	160
i. Reflexion, Ganglia, and Man's Immortal Soul .....	161
ii. A Mental Pathology .....	181
iii. Mind and Organism .....	195
iv. Mind and Cosmos .....	205

<b>Chapter Five: Evolution, Ontogeny, and the Natural System .....</b>	<b>214</b>
Introduction .....	214
i. Homology, the Archetype, and the Evidence of Embryology .....	216
ii. Huxley's Synthesis .....	222
iii. Darwin and the Natural System .....	238
iv. "Embryos and Ancestors" .....	246
v. Cambridge Embryology, 1870 - 1890 .....	259
vi. The Mechanism of Development .....	267
<b>Chapter Six: Naturalism and Society, 1880 - 1914 .....</b>	<b>271</b>
Introduction .....	271
i. The Passing of the Old Order .....	276
ii. The New Teleology .....	280
iii. The Constraints of Nature .....	290
Concluding Remarks .....	310
<b>Appendix A: The Huxley Circle .....</b>	<b>317</b>
<b>Appendix B: Glossary of Zoological Terms Used in Chapter Five .....</b>	<b>322</b>
<b>Bibliography .....</b>	<b>323</b>
<b>References .....</b>	<b>363</b>

## Introduction

In the historical retrospect of any period, the relation between its Thought and Action becomes clear; and its philosophy appears, no less than its poetry, its art, or even its polity, distinctly expressive of its real inner life.<sup>1</sup>

(James Martineau)

Ideas are only a partial aspect of a less abstract reality: that of the whole living man. And in his turn, this man is only an element in a whole made up of the social group to which he belongs.<sup>2</sup>

(Lucien Goldman)

This thesis is a study of scientific naturalism in Victorian Britain from a particular standpoint. This introduction is intended to define both the object and the method of enquiry.

In a sense 'naturalism' had to be invented: it was largely an artefact of a group of commentators on recent science and philosophy at the end of the nineteenth century. They looked back on the events of the last fifty years and isolated certain tendencies in thought. They compounded these trends into a fully-fledged world-view called scientific naturalism. It was admitted that few, if any, had ever articulated the full naturalist doctrine; moreover, some of those who were dubbed naturalists strenuously denied the label. Nonetheless, it was maintained that the concept of naturalism captured an important movement in contemporary culture.<sup>3</sup>

A similar procedure is followed here. This study detects a common drift in a variety of utterances and actions in the later Victorian period and comprehends them under the term 'naturalism'. This notion is chiefly a heuristic device designed to combine a number of events for joint consideration. To some extent the definition of naturalism adopted here is an arbitrary one: it involves a decision as to what naturalism denotes and connotes. This decision was taken to signify the main lines of the argument.

These can be reduced to three. The first, and most important is to regard naturalism as a strategy; that is, to see it as contributing to an end. The second is to isolate certain structures which pervaded the naturalist cosmology. The third aim is to show something of the provenance of naturalism, and especially to demonstrate how it resembled previous cosmologies in certain respects. These three features of naturalism can be indicated in a preliminary way by the writings of some of

those who coined the term.

In 1903 G. E. Moore published an account of naturalism as a move in the game of ethical theory. Moore provided both a generic definition of the kind of procedure in moral philosophy to which naturalism belonged and a statement of its specific characteristics. All ethical theories, he argued, committed the 'naturalistic fallacy' whenever they confused the factual question 'what can be called good?' with the normative judgement 'by what means shall we be able to make what exists in the world as good as possible?'. In other words, they identified some object (complex or simple) as **being** good, and proceeded to order other objects according to their potential to conduce to 'the good'. From this scheme of things they derived moral imperatives that enjoined actions which would enhance the incidence of the good and which proscribed those that diminished it.<sup>4</sup>

Moore included several types of ethical theory in this general conception. For instance, idealism, as expounded by F. H. Bradley, was guilty of the naturalistic fallacy because it identified the 'Absolute' as the good, and urged men to regulate their actions so as to realise the conceptual form of the Absolute in society. Moore also drew attention to another form of the fallacy, which had been a feature of the recent past, whose method consisted in

substituting for 'good' some one property of a natural object or of a collection of natural objects; and thus replacing Ethics by some one of the natural sciences.<sup>5</sup>

Regarded as a strategy whose reference was much wider than the field of academic ethics the chief characteristic of naturalism was therefore its use of nature to generate a 'moral order' — a system of preferences which were supposedly binding upon society. The forms which the injunctions derived from this conception of nature took were more often prudential than deontological: they drew their force from the supposed practical consequences of compliance and non-compliance rather than from an appeal to some 'categorical imperative'.

Regarded as a cultural event, Moore recognised that naturalism involved the attempt to aggrandise natural science at the expense of more traditional forms of knowledge. The 'nature' by which human affairs were to be regulated was no more than 'the subject-matter of the natural sciences and also of psychology'; the doctrine had been preached most forcibly by such Victorian prophets of science as John Tyndall and W. K. Clifford.<sup>6</sup>

James Ward had also perceived an element of cultural imperialism in the articulation of naturalism. He noted a tension between the avowed desire of scientists to demarcate an area of special professional competence, exclusive of metaphysical questions, and the energy with which some of them pressed the relevance of science in all areas of human concern. They had attempted to distinguish 'town' — the scientific — from 'country' — the rest of culture, but, Ward complained,

where is science to end? All was country once, but meanwhile the town extends and extends, and the country seems to be ever receding before it.<sup>7</sup>

Ward went on to detail the main elements of the world-view whereby science was assigned such an extensive scope. It could, he maintained, be resolved into three fundamental doctrines:

(1) the theory that nature is ultimately resolvable into a single vast mechanism; (2) the theory of evolution as the working of this mechanism; and (3) the theory of psycho-physical parallelism or conscious automatism, according to which mental phenomena occasionally accompany but never determine the movements and interactions of the material world.<sup>8</sup>

The second and third of these theories can be seen as inferences from the first.

The idea that the universe was a machine could be justified in terms of the major triumph of nineteenth century physics, the Laws of Thermodynamics, which were themselves based upon the actions of man-made engines. According to the first of these laws, the quantity of energy in the universe was constant; while 'the energy of any given body or material system may vary indefinitely', every such variation involved the transformation of a quantum of energy from a potential to a kinetic state, or vice versa; alternatively, it could be effected by the transfer of energy to or from another system.<sup>9</sup> This mechanism explained all movement and change in nature: no other agency was either necessary or possible.

The doctrine of psycho-physical parallelism, which held that states of consciousness were incidental to bodily movements, could be viewed as an application of this principle. Energy, the capacity to do work, attached only to material systems; by definition, therefore, it could not be the property of an immaterial state of mind. This restriction applied not only to the human psyche: it denied any causal potency in the physical world to 'spirit' of whatever kind.

The second law of thermodynamics held that there was a secular tendency discernible in the condition of the energy in the universe: in the course of time progressively less was available for doing work. In contrast to classical mechanics, therefore, nineteenth century physics possessed a temporal element: not all processes were perfectly reversible; within the local vicissitudes of energy systems was an overall tendency in one direction.<sup>10</sup> In this sense there was an 'evolution' in the working of the cosmic mechanism.

However, in nineteenth century Britain 'evolution' usually meant organic evolution. In this context naturalism made a different use of the concept of 'mechanism'. The theory of natural selection was mechanistic because it asserted that evolution could be comprehended without resort to the categories of design and purpose. Naturalism in this and in analogous cases repudiated teleology, and held that all valid explanation in science must rely on an account of the physical devices whereby a particular outcome was achieved.

Despite the professed dependence of these doctrines upon the novelties of nineteenth century science — upon the physics of Clausius, the physiology of Helmholtz, and the biology of Darwin — Ward saw a continuity between Victorian naturalism and the speculations of earlier ages. Scientific naturalism, he remarked, 'as regards its exclusion of everything supernatural or spiritual', strongly resembled the naturalism of the seventeenth century which also denied the 'existence of things divine or spiritual', and dogmatically asserted that 'matter was the one absolute reality'.<sup>11</sup>

R. Otto, writing in 1907, went further in tracing the ancestry of nineteenth century naturalism. Its basic assumptions were, he held, 'as old.....as philosophy — as old as human thought and doubt'. Throughout history the watchwords of naturalism 'remain the same, though in an altered dialect: "nature and natural phenomena", the denial of "dualism", the upholding of one principle "monism", the all-sufficiency of nature, and the absence of any intervening influence from or beyond nature'.<sup>12</sup> Ultimately, naturalism sought to devise

a cosmos of all Being and Becoming, which can be explained from itself, and comprehended in itself alone, supported by its own complete and all-sufficing causality and uniformity, resting in itself, shut up within itself — a God sufficient unto himself and resting in himself.<sup>13</sup> This isomorphism between nineteenth century scientific naturalism and the pantheism and

materialism of earlier epochs itself reflected common polemical interests which such ideas had subserved in a variety of contexts. The most important of these was the rejection of the dualism between mind and matter which lay at the heart of Christian orthodoxy. The origins of this monism could be traced to the classical hylozoists, Democritus and Lucretius, and thence to the writings of such Renaissance thinkers as Giordano Bruno.<sup>14</sup> However, Leslie Stephen chose to take Spinoza as the most important source of the doctrine. While Descartes had distinguished three substances, God, soul, and matter, the first two of which were closely akin but totally different from the third; Spinoza had recognised only one.

God, let us say, is the sole substance of the universe; he has infinite attributes; the soul is God, known under the attribute of thought; and matter is God, as known in the aspect of extension.<sup>15</sup>

There was a single underlying cause of which the spiritual and the material were aspects.

Such a doctrine was usually described as pantheistic, but it was equally just, Stephen argued, to call Spinoza an atheist, 'if **Atheism means a mode of conceiving the universe which is radically inconsistent with the old theology**'. The most vital effect of monism was to abolish the 'God of the churches' who

is separate from the universe; he must punish and reward, create and destroy, and interpose at intervals to alter the working of the established order. The conception disappears equally whether the existence of God or not-God be denied. The divine power seems to be a factor which enters on both sides of every equation, and may therefore be omitted ..... The God of Spinoza is pure Being; and though Spinoza retains for this abstraction the reverence due to the concrete Person of popular theology, and exhibits his doctrine as a system of ethics, the ordinary mind fails to regard his deity as an object capable of exciting emotion or guiding conduct.<sup>16</sup>

The progression was therefore from cosmology to theology to ethics: to rival attempts to frame a concept of moral order. The negative effect of naturalism was to vitiate schemes which founded ethics upon the will of a transcendent ruler of the world by denying that any power or mind lay beyond nature. Positively, naturalism held that ethics must be based upon the recognition that humanity was



fully endogenous to the material world. In previous contexts, such as seventeenth century England and eighteenth century France, these resources had formed part of a politically motivated attack upon 'old theology' and the priestly establishment it represented. An explanation of scientific naturalism must also include an exposition of the interests in Victorian Britain that were inimical to the persistence of the 'God of the Churches' and of the power of his earthly ministers.

A number of commentators noted that, despite the affinities between nineteenth century naturalism and that of an earlier era, the former was in some respects significantly different. Ward, for instance, remarked upon a tendency for naturalism to detach itself, at least partially, from ontological questions and to concentrate on epistemology and methodology.<sup>17</sup> Ralph Barton Perry in 1912 made a similar observation; there were he argued two forms of naturalism. The older of the two searched for 'a universal substance and a first cause, and claims to have discovered these in some such scientific concept as "matter" or "force".' The second kind of naturalism was of more recent provenance; it

condemns the search for universal substance and first cause as futile. Its last word is a theory of knowledge, in which science is asserted to be final because the only case of exact knowledge. In other words, the second variety of naturalism claims less for the concepts of science, but nevertheless claims all.<sup>18</sup>

The distinction between the two naturalisms might be further specified as that between 'materialism' and 'positivism', or between a 'naturalism of substance' and a 'naturalism of method'. While 'positivism' captures the sensational and nominalist epistemology which was used to justify this shift away from things in themselves to sensations and categories, the notion of a 'naturalism of method' touches upon the increasingly specialised methodological concerns to which naturalism tended in the later nineteenth century.

In general terms, naturalism asserted the possibility of an autonomous account of phenomena which made no reference to the influence of supernatural powers in the universe because such were, *ex hypothesi*, excluded. Moreover, it identified the 'material', or (in the positivist version) the empirical, as the source of all events, and, therefore, as the proper locus of study. More precisely, scientific naturalism imposed a classification upon the world which further orientated attention and

which suggested research strategies.

The cosmological principles of naturalism could be translated into negative or positive recommendations about how to proceed in a given science. One such negative injunction has already been noted, that which denied teleology any status in scientific explanations. Psycho-physical parallelism, on the other hand, could yield the positive recommendation to concentrate solely upon the physiological aspects of sensation and movement to the exclusion of the states of consciousness which might accompany them. The theory of evolution maintained that, for taxonomic purposes, organisms should be regarded as in a temporal continuum with genealogical affinities.

Such features of naturalism need to be referred to a growing sense in nineteenth century Britain of science as an autonomous intellectual activity. There was an evident concern to establish the relations of natural knowledge with other aspects of culture, as well as to decide the proper basis of particular disciplines. In effect, naturalism was an aspect of the professionalisation of British science in the Victorian period.

This notion of naturalism indicates the main issues to be discussed below. It also suggests something of the explanatory strategy adopted. Scientific naturalism is viewed as part of the symbolic resources of Victorian culture. In common with the rest of culture it was constructed under determinate conditions. The crucial contention of this thesis is that the cultural product cannot be understood in isolation from the context of production.

The distinction between culture and society is assumed to be a provisional one. Culture in all its forms emerges in the course of social activity. It is not, however, 'epiphenomenal', in the sense of passively mirroring social reality; rather, its making is internal to the interaction of social groups. The interests which the participants bring to this interaction shape the world-view of a historical period. Cosmology is the symbolic embodiment of the goals of historical actors.

Various theoretical materials have been used in developing this approach. The work of such philosophers and historians as Quentin Skinner has drawn attention to the possibility of assimilating 'utterances' — that is, all forms of symbolic exchange — to action. Thereby cultural products can be referred to the intentions of their authors. In particular, utterances serve to communicate meanings

between members of a society in the hope of influencing the actions of others.<sup>19</sup>

This type of intellectual history has tended to be atomistic in its explanations. Attention has focussed upon individual goals as most relevant to the understanding of utterances. However, the proponents of this approach have acknowledged the need to ascertain the context within which an author acts in order to account for his communicative intention. To quote Skinner, it is necessary to grasp 'what sort of society the given author was writing for and trying to persuade.'<sup>20</sup>

While individualism is characteristic of the history of ideas, sociology and social anthropology have emphasised the importance of 'structural' explanations of cultural productions. These bypass individual motives and concentrate on features of social structure to which a given class of utterance can be referred. It is argued that the relations between social groups generate a variety of interests; these interests, rather than individual motives, are the instigators of communicative action.<sup>21</sup>

The latter approach greatly expands the explanatory resources available to the historian. It also helps to assimilate intellectual history to other branches of the discipline in both form and content; 'structural' explanations are commonplace in the history of international relations, for example. But it also poses difficulties. Individuals' utterances are the historian's data; while he may aggregate them into larger units, individuals remain the chief object of his attention. This difficulty is not insuperable, however: there is no necessary antithesis between individualistic and structural explanation. In Mannheim's words,

To recognise that the individual is the focus of reality is not the same as to construe the self as an isolated entity: to understand his behaviour one has to know the constellation in which he acts.<sup>22</sup>

This constellation is equivalent to the social structure which comprises an individual's historical reality.

Other social theorists have tried to show how such an integration of individual and social can be accomplished. Notably, Lucien Goldmann argued that 'exceptional individuals' articulated the representative cosmologies of social groups. The cosmology was representative, not in the sense that it portrayed the views of all or most of the members of that group, but because it expressed in a particularly clear way the social situation of a fraction of society. Thus the tragic vision of Pascal and

Racine was the purest statement of the dilemma confronting the 'Noblesse de Robe' in seventeenth century France.<sup>23</sup>

Goldmann's account tends towards a passive interpretation of the relation of cosmology to social reality. However, a still more thoroughgoing erosion of the distinction between individual and societal can be attained if a dynamic and instrumental view of culture is taken.

As Giddens has noted, the concepts of 'motive' and 'interest' are closely related.<sup>24</sup> Both are 'wants' which can be satisfied in particular ways. In one case the wants are attributed to an individual, in another to a collective; but there is no difference in kind between them. On the contrary, individual wants cannot be construed independently of societal considerations. The notion of the individual is itself dependent on such structural contributants as class, occupation, family position and other forms of social status. The 'individual' is constituted by a collection of such roles; as a result his wants cannot but express interests inherent in the social structure.

However, there is no symmetrical relation between individual utterances and social location. Not all individuals in the same social situation express the same views. Nor does an individual necessarily express views that can be associated directly with his position in society. The reality is much more complex. This thesis, together with the sociological literature cited above, maintains that certain individual utterances can, nonetheless, be considered as the representations of particular interests. The problem of establishing this in any given instance is an historiographic rather than a theoretical one. But an understanding of how utterances can contribute to the attainment of the goals of groups requires a concept of the general character of social interests.

Durkheim held that social phenomena were essentially 'moral': they implied constraints on action which could be expressed in prescriptive injunctions.<sup>25</sup> These imperatives did not necessarily take an overt form, but every area of social practice implied a normative order. Moral in this sense connotes that an unequal preference is assigned to a range of possible states of affairs. To say that society is moral is therefore to say that it embodies some hierarchical concept; in particular, the 'social order' incorporates a differentiated access to resources. It also implies that all institutions are biased to facilitate some courses of action and to inhibit others. To say that a system of ideas is moral is to say that it commends one notion of hierarchy.

In even a moderately complicated society many moral orders will be possible. Some will assign a different share of burdens and benefits to the various social groups; others will impose a particular orientation upon areas of social action. There will be at least as many moral schemes available in the culture. A group's interest lies, ultimately, in the achievement of the social order which maximises the benefits and minimises the burdens upon it. The maintenance of institutions upon a given course or their deflection onto a new one is the chief means whereby such advantage is sought and gained.

All other interests can be seen as instrumental to these efforts to define social relations in a way most advantageous to a group. Cultural symbols serve some of these instrumental interests. Because the achievement or defence of a hierarchy involves the eliciting of responses both within a group and from other groups, it also entails **communication**. Demands must be transmitted and responses received in the course of negotiating social order. The 'communicative' interest in society is thus subsumed within the wider material interests of its elements: the transmission of meanings forms an integral part of the competition between social groups.<sup>26</sup>

This communication must proceed through individuals; however, for reasons outlined above, it would be a mistake to regard individual utterances merely as the expression of singular motives. Instead the individual may, in principle, be seen as the medium through which a social reality becomes articulate. In practice, the historian is confronted with the problem of showing how a particular utterance or group of utterances can be seen as the expression of a particular interest.

To do this it is necessary to reveal the context in which the utterance occurred. Each statement needs to be viewed both temporally, as an incident in a dialogue between several parties; and as part of a complex of ideas, persistent through time, in which the meaning of each is determined by the meaning of the rest. Moreover, whether viewed as series or structure, these symbolic artefacts are features of a larger totality: the political, social and economic life of an historical epoch. The elucidation of how a class of utterance was connected with an interest requires the demonstration of how they coexisted as aspects of this whole. In short, the way in which culture is created and used in and through the material conditions of the time must be described.

The idiom in which the members of a society choose to communicate is contingent upon the cultural resources available to a society. In both 'advanced' and 'primitive' societies the natural world

has been an especially important means of transmitting demands and exhortations. Why this should be is unclear; all efforts at an answer plunge into speculation about the 'collective consciousness'.

However, the fact that the events discussed in this thesis form part of wider historical pattern is important; it illuminates both what was new about scientific naturalism and what had parallels in earlier periods. In brief, scientific naturalism represented a late instance of a cosmological strategy which had occurred in Britain and elsewhere in both the seventeenth and the eighteenth centuries. On the other hand, naturalism embodied features which had not appeared in the earlier cases or had been less important. While the persistent characteristics may be referred to certain similarities between the social context of, for example, late eighteenth century France, and mid-Victorian Britain, the novelties must be related to the special features of nineteenth century British society.

This account makes no prior judgment about the nature of the interests which a given cosmology serves nor about the level of social analysis appropriate in any case. Instead it insists that the interests of any social unit may be relevant to the understanding of a particular piece of culture. The significant interests may, in one case, pertain to a class or class-fraction; in another to a professional group or to a narrower specialty. In each case it is necessary to show how a particular representation of reality was relevant to the achievement of a defined interest. It is only when such investigations in detail have been made that broader patterns emerge.

The argument of this thesis is that the naturalist world-view served the interests of sections of the 'new' professional middle-class in the later nineteenth century; it also suggests that intimations of this correlation can be seen earlier in the century. In particular, scientific naturalism was an aspect of the professionalisation of science in the period after 1850. This, in turn, was part of a more general differentiation of professional groups in Britain during these years. These structural changes generated a variety of interests: of special importance were the efforts of the new professionals to negotiate or renegotiate fields of activity which they could claim as their own. It was in these contexts that the utility of naturalism to their goals emerged.

The thesis is organised by topic. I have isolated several areas of cosmology and tried to show how, in each case, particular accounts of the world conduced to certain social interests. To a large degree each chapter is self-contained; however, there is a continuity between them. In part, this is a

unity of material: the same conceptual structures occur repeatedly. In part, it is a unity of personnel: the same individuals were active in several of the contexts described. Underlying this unity of resources and actors is an interpretative unity: the claim that all of these instances illustrate part of the same social movement.

The choice of topic was mainly determined by contemporary preoccupations. The controversies of the period tended to centre upon questions such as the nature of life and to return, by however tortuous routes, to the relation of the spiritual to the material. Given these general areas of concern, it was still necessary to decide how to treat them. The criterion used in making such judgements was to choose aspects of each topic which would, cumulatively, illuminate the main features of the strategy of scientific naturalism. This involved a similar treatment of some subjects, such as life and cause, to show how comparable interests informed different themes. In the same way, it would have been possible to describe the way in which evolutionary notions were used in debates about the proper scope and cultural role of science. However, instead I have used the chapter on evolution to exemplify another side of scientific naturalism: the process of 'paradigm-building'. At first sight, the relatively esoteric issues involved in this activity may seem distant from the public controversies outlined elsewhere. But the two were complementary parts of the same process; both served, in different ways, the constitution of science as a profession in Victorian Britain.

The organisation used here has posed some problems. One is the difficulty of deciding whether to rely on an argument which is made elsewhere or whether to reprise some point *in situ*. This dilemma has proved especially acute in the case of contextual material. In general, I have assumed a knowledge of what has been said in Chapter One; but in some instances I have elaborated points made there in subsequent discussion to show their application to a particular instance.

Another problem has been to decide how wide or how narrow a net to cast. On the one hand much fascinating material has been omitted because it does not contribute directly to the main argument of the thesis. On the other, I have felt it necessary in some cases to include events which fall outside the principal focus of the enquiry as well as to give some consideration to anti-naturalistic utterances. The justification for this is that scientific naturalism cannot be understood in isolation. The naturalistic cosmology emerged through a dialectical process in opposition to other representations

of reality. Moreover, the episode of the 1860s and 1870s was a moment in a much more extensive confrontation. A complete account of naturalism would have to show these historical relations. Such completeness is unattainable; however, I have tried to reveal something of that wider perspective.

This discussion of naturalism, of course, does not pretend to be comprehensive: no history ever is. Further, all history is written with particular aims which divert attention from some aspects of a subject and direct it to others. This study differs only in that it makes its aims explicit. Given different explanatory interests a divergent account of scientific naturalism could be written which would stress other aspects of the topic. Any historical period is infinitely complex; all that any description can do is to present one way of looking at it.

My general object has been to show how the study of cosmology, including what is conventionally distinguished as 'science', can be incorporated into a wider historiographic project. Thereby both the history of science and general political and social history will be enriched. The essential assumption which informs this approach is that distinctions between 'thought' and 'action' are abstractions from a concrete historical reality. In that reality, all culture and all knowledge is part of a dynamic whole whose unfolding is equivalent to the making of history. Analysis of that whole is indispensable, but the most satisfying explanation is that which attempts an eventual synthesis; which seeks to return what has been abstracted to the nexus of relations in which it was generated. It is the potential of such explanation that I have tried to demonstrate here.



## CHAPTER ONE: The Context of Naturalism

### i. The Strategy of Naturalism

'During the nineteenth century the intellectual activity known as science was transformed into a profession'. The general characteristics of this transformation which occurred throughout western Europe and in the United States were a growing sense of group identity among scientific workers, expressed in a proliferation of organisations to represent the views of 'Science' to the nation; greater specialism; and the expansion of employment opportunities.<sup>1</sup> In each nation, however, the rate at which these developments occurred and the special form that they took were conditioned by the particular conditions within which professional science had to establish itself.<sup>2</sup>

In Britain a variety of measures of the progress of professionalisation are available. In the 1841 census, less than 1,000 people gave their occupation as 'Literature or Science'; in 1871 the figure had risen to 7,000, and by 1881 to 9,000. This total does not discriminate between those employed in natural science and those in the arts; it does, however, indicate the establishment of a class of professional intellectuals in Britain during this period.<sup>3</sup> The growth of science as such is more clearly seen in the expansion of scientific societies in the course of the nineteenth century: in 1760 there was 12 formal bodies concerned with science; by 1870 there were 125. Between 1850 and 1889, the period of greatest growth, membership of the major of these societies rose from 4,597 to 12,314.<sup>4</sup> The census figure does not, therefore, reflect the true extent of the scientific profession in late-Victorian Britain; many of those who gave their occupation as 'education' or 'government' were, by their membership of a professional association, 'scientists'.

In education, the number of teachers of physics and chemistry in British higher education stood in 1850 at 34; in 1880 it was 109. Numbers of students also rose, as did that of institutions offering instruction in science. In the third quarter of the nineteenth century technical colleges were opened at Newcastle (1871), Leeds (1874), Sheffield (1874), Birmingham (1880); these augmented the efforts of their Manchester prototype. University Colleges with a scientific and technical bias were opened at Nottingham and Liverpool in 1881, again following a Manchester lead. In London, University and King's Colleges had since the 1830s offered science courses as part of medical training. These were supplemented in the second half of the century by the Royal School of Mines and the

School of Chemistry, which united to become the Normal School of Science at South Kensington in 1872. As a result of this growth, the number of science graduates in Britain rose from 77 in 1870 to 346 in 1880; by 1910 the total had reached 10,910.<sup>5</sup>

These figures indicate the trend towards the creation of a scientific profession in Victorian Britain; they mark successive stages in the process. They do not, however, reveal much about the nature of that process: they do not show **how** science was transformed in these years from being predominantly an activity performed incidentally by individuals who drew their income from some other source into a profession with considerable institutional foundations. This transformation needs to be viewed, in part at least, as an achievement of a social group operating within certain restraints and with certain resources; among these resources were the cosmological principles of scientific naturalism.

'Professionalisation' is an abbreviation for a number of related intellectual and social changes in the status of science; it is important to recognise the interdependency between these two kinds of reformation. Essential to the cognitive changes involved in professionalisation is 'the gradual shift from natural knowledge seen as areas of enquiry subsidiary to other social pursuits ..... and other intellectual concerns (such as theology) to that same knowledge seen as a collection of independent mental disciplines, each with its own particular tools, techniques, traditions, and central problems.'<sup>6</sup> This can be regarded as a process of **definition** whereby a field of competence exclusive to science is designated and a division of labour among scientists arranged.

In the course of the first, external, definition, it becomes necessary for science to negotiate its relations with other groups. In particular, the special territory to which its members lay claim may impinge upon that of other groups, and this may lead to conflict. In these circumstances, the drawing of cognitive boundaries is equivalent to determining the frontiers between social groups: the way in which the world is classified has certain implications for the competences and authority enjoyed by different professions.

The task of 'internal' definition is equivalent to the manufacture of the intellectual bases, or 'paradigms' of disciplines. A paradigm, as defined by Kuhn, is a normative structure that commends certain orientations towards the study of some aspect of nature at the expense of others. The form

of this commendation varies: it may be embodied in explicit textbook axioms or in an exemplary piece of work; or, more usually, in both these ways and in the multitude of covert discriminations and choices embodied in every system of scientific education. What matters is the intimate fit between theory and practice; indeed the two are inseparable under the ideal conditions of 'normal science'. The practice assumes certain propositions about the constitution of the world, and about how knowledge about certain features of nature is best to be attained. These constitutive principles of a discipline are rarely, if ever, articulated. The analogy with Wittgenstein's notion of 'rule-governed' behaviour is strong: under the conditions of normal science, those working within a paradigm may not 'know' the rules according to which they operate in any explicit sense; but the reality of those rules is evinced in all their actions.<sup>7</sup>

However, Kuhn recognises that such episodes form only one moment in the history of a discipline. Periods of normal science are preceded by 'pre-paradigmatic' eras in which no moral order is dominant and embodied into institutional practice. At this stage, the paradigm is far more an overt set of recommendations than it later becomes; it may be viewed as a programme or a manifesto in which a certain kind of science is preferred to its rivals. Such programmes include the cosmological and metaphysical tenets which the established paradigm assumes, and justifies their adoption in terms of the benefits that will accrue from the pursuit of a given line of research.

'Normal' and professional science are in key respects similar. In both cases, science proceeds through established institutions: indeed, the science is an institution, in that it is carried on as a routine occupation of a group of workers holding formalised roles in the social structure. Normal science presupposes standardised procedures of training, established hierarchies, recognised research orientations and methods, and secured resources — all of which are also the characteristics of a professional discipline. In contrast, 'pre-paradigmatic' science is also pre-professional. The conflict between paradigms corresponds to the period of flux prior to the professionalisation of a science in which the conditions — the assumptions and practices — of normal science are being **created**. The criterion for paradigm choice is precisely the extent to which any given system conduces to the achievement of a professional matrix for a discipline.

In nineteenth century Britain scientific naturalism served to define both the internal and

external boundaries of the new profession of science. The unity and self-sufficiency which a materialistic monism imposed upon nature corresponded to the claim that an autonomous scientific enquiry was possible; thereby the ambit of the scientific profession was defined over and against that of other groups. Secondly, particular items of the naturalistic world-view yielded a putative framework for sciences which were still underdeveloped and which lacked any strong directing principle; this was especially the case of the life sciences in mid-Victorian Britain. In short,

the transformation of the epistemological status of science from that of "Natural knowledge" occupying a minor, if honoured, position in literate culture, to that of an autonomous source of authoritative understanding not only of natural phenomena but of the social environment as well

was

the result of the creative effort of scientists in extending the coherence, specificity, predictive ability, and explanatory power of the materialistic paradigm in science, and challenging the predominantly theological and philosophical consciousness of traditional culture.<sup>8</sup>

This effort occurred in a context where other groups, especially the clergy, with a stake in retaining the 'predominantly theological and philosophical consciousness' in the study of nature, resisted the establishment of the 'cultural apartheid' which was one of the conditions of the professionalisation of science. But if one use of naturalism lay in this definition of a cognitive sphere for natural science independent of 'non-professional' concerns, a countervailing tendency was also evident. While professionalisation demanded that science be made the esoteric specialty of a narrow sub-culture, it was also necessary that science become a power in the wider world if the more general social conditions of professional status were to be secured.

In particular, science as a profession was only possible if jobs became available for its members. But scientific institutions would only be created if wealth were diverted from other uses: science could only prosper at the expense of others. To succeed science therefore had to become a political force; in a 'world of scarce resources, a profession can only grow to the extent it is successful in capturing social support among strategic interest groups.'<sup>9</sup> Such allies would only be forthcoming if

the utility of science to their own interests could be shown; in this context, the strategy of scientific naturalism, considered as an instrument of the scientific profession, also became the property of more extensive social movements.

At one level, the search for allies took the form of marketing scientific knowledge to groups that might become significant consumers, and so supply some of the funds needed. At another, it involved participation in the political conflicts of the period, in the assault upon the position of established groups, in the hope that science would enjoy some portion of the spoils of victory. In the former effort, naturalism provided a resource for asserting the extensive relevance and instrumental power of scientific knowledge. In the latter, naturalistic conceptions of reality assumed an overtly factional and class significance which was, to a considerable degree, the continuation of the former political and social meaning of such cosmologies.

The remainder of this chapter elaborates two of the naturalist strategies that have been outlined. The circumstances in which the 'external definition' of the province of science occurred will be considered; then the material resources potentially available to science and the attempts that were made to obtain them will be described. Both exercises involve an account of the relations between new and old elites in Victorian society; special attention will be given to the competition among these for control of education. Finally, the political combinations into which the champions of professional science entered to gain a foothold in this field will be discussed, together with the use of naturalism in the larger movement in which science was thereby implicated. Consideration of the third naturalist strategy, that of 'internal definition, is relegated to the body of the thesis.

## ii. The Bifurcation of Science and Theology

Extinguished theologians lie about the cradle of every science as the strangled snakes beside that of Hercules.

(T.H. Huxley)<sup>10</sup>

James Ward noted that one of the characteristics of nineteenth century naturalism had been to narrow the range of concerns that were relevant to scientists. In particular, theological questions were ostentatiously excluded from consideration. Science was not concerned with ultimate questions: if it had a voice, Ward claimed, 'and were questioned as to this omission of all reference to a Creator', modern science would declare, 'I am not aware of needing any such hypothesis.'<sup>11</sup>

T.H. Huxley in 1868 had argued that such a process of exclusion was intrinsic to the secular development of science. It entailed the rejection, a priori, of certain kinds of concepts and explanatory resources:

Anyone who is acquainted with the history of science will admit, that its progress has, in all ages, meant, and now more than ever means, the extension of what we call matter and causation, and the concomitant gradual banishment from all regions of human thought of what we call spirit and spontaneity.<sup>12</sup>

Such crypto-Comtean formulations obscured the true nature of these distinctions, however. God was withdrawn from the class of objects of scientific concern and spirit from that of possible causal agents not as part of the inexorable development of scientific thought, but as a move in a contest between old and new views of the cultural function of natural knowledge.

There was a long tradition in Christian thought which looked to nature as a source of information on the nature of God and of his purpose for man. As Thomas Laycock put it in 1860, *Speculative Theology*

has always sought to draw to its support arguments from special science or philosophy; and for the very obvious reason that some of the most important dogmas of the Christian Churches ..... are intimately bound up with metaphysical speculation.

However, such theology approached cosmological and metaphysical questions with the preconceived notions of dogma, and the former were reshaped to be compatible with the latter. Hence, Laycock

concluded, 'to the true ecclesiastic, philosophy is not an *inquisitor veri*, but a *confirmatio veri*.<sup>13</sup>

The form that such 'natural theology' took varied, as did its professed relation to other sources of religious inspiration such as Scripture and Church tradition. In nineteenth century Britain two major types of natural theology can be distinguished, although these categories subsume a much more complex pattern of difference and affinity.<sup>14</sup>

The first of these was articulated chiefly by a relatively small group of Anglican divines and gentleman scientists. Aware of the theological odium that had descended on science in early nineteenth century Britain, these tried to demonstrate the perfect compatibility between science and Christian belief, and had argued that, when properly guided by sound theological principles, the former revealed the reality of divine power in nature and refuted all infidel arguments. In effect, Whewell and Sedgwick at Cambridge and Buckland at Oxford perpetuated a sophisticated type of natural theology which could be traced back to the writings of Newtonian divines at the turn of the seventeenth century. They were aided in their efforts by such lay scientists as Charles Bell and, to a lesser extent, John Herschel. The great monument to this loose association of like-minded workers was found in the majority of the 'Bridgewater Treatises', a comprehensive statement of the evidence of God's will and providence in his creation.<sup>15</sup>

Far less elaborate, and far more prevalent, was a view of the theological significance of nature held by the evangelical sects and by considerable fractions of the Church of England. These were wedded to Biblical fundamentalism, and held that a 'literal' reading of Scripture supplied the sole source of theological authority. Because the Bible contained certain cosmological claims — for instance, the account of creation found in 'Genesis' — these too were divinely inspired and necessarily true. Any theory which deviated from the model of nature derived from the Bible was therefore at least potentially blasphemous.

The consequence of these theological concerns were two-fold. The search for God as agent and planner in nature meant that scientific questions continued to be entangled in those of theology and philosophy. This conflated the scientific and clerical roles and militated against the establishment of the notion of science as a separate occupation. Secondly, as long as nature remained an object of theological interest, it was open to the cleric to intrude into scientific considerations and to insist that

the principles of his profession be given due weight in any discussion; he might even demand that theological considerations be the determining criterion in issues of cosmology and that science must defer to these.<sup>16</sup>

The clergy remained in Victorian Britain a powerful group able to mobilise forces of coercion against those who were too 'heterodox'. Their chief weapons were ridicule and abuse conducted through a multitude of religious periodicals; the effect of such polemic generally impinged even upon the most freethinking scientist, if not directly then through family pressure to conform. The result of such intimidation was that a

thoroughly naturalistic approach to the investigation of the universe was thwarted by considerations that had no intrinsic relationship to the undertaking .....Scientific research stood subordinate to moral values, a concept of God, and a view of human nature that had been formulated by clergy and religious writers.<sup>17</sup>

The most notorious example of theological intervention to suppress unorthodox beliefs came early in the century when William Lawrence's premature attempt to detach physiology from philosophical and theological concerns and to wed it firmly to the practical interests of medicine was met with a concerted attack. Lawrence withdrew in the face of a vitriolic invective; however his fate was not forgotten. As late as 1893, Huxley angrily recalled that it 'was not so very long ago that my kind friend, Sir William Lawrence, one of the ablest men whom I have known, had been well-nigh ostracised for his book **On Man**'.<sup>18</sup>

Later in the nineteenth century, a similar pressure was still exerted against scientific thought. An outstanding instance of this is found in Darwin's reluctance to publish his evolutionary theories for fear of persecution. He was obliged, until 1859, to maintain a barrier between the 'public' knowledge that it was safe to make generally known, and the 'private' which could be entrusted to only a tiny circle of discreet friends.<sup>19</sup> Charles Lyell too, in order to ensure an appointment at King's College London, had to represent his geological opinions as compatible with Scripture, because the Bishop of Llandaff had insisted that Lyell's views 'should be confined within the limits prescribed by theology'.<sup>20</sup> Several years later Lyell commended Buckle's anti-clerical **History of Civilisation in England** to George Ticknor with a bitter jibe against 'That large party here who exert a severe censorship on all who dare



to think differently from the "endowed doctrines".<sup>21</sup>

Even the orthodox Richard Owen was subject to similar control. He was censured in the religious press in 1849 on the grounds that his morphological views conduced to atheism; in response to family representations he published a rapid denial and insisted upon the religious respectability of his science.<sup>22</sup> Despite this show of compliance with clerical demands, Owen took the opportunity in 1864 to 'treat the parsons so contemptuously', that Lyell was sufficiently surprised to inform Huxley of the fact.<sup>23</sup>

Owen's response to clerical pressures was not untypical; many others combined a show of submission with occasional outbursts of resentment — usually in private. It was received wisdom among British scientists that particular subjects involved 'burning questions', in the sense that 'those who touched them were certain to burn their fingers severely'.<sup>24</sup> Among these questions was that of the zoological classification of man: when in 1862 Huxley sent the proofs of his **Man's Place in Nature** to 'a highly competent anatomist' for comment, his friend expressed a high opinion of the work. He nonetheless counselled Huxley, for the sake of his welfare, not to publish it. Huxley ignored this advice, but noted:

I must do my friend the justice to say that his forecast was completely justified. The Boreas of criticism blew his hardest blasts of misrepresentation and ridicule for some years, and I was even as one of the wicked.<sup>25</sup>

Such individual acts of defiance came to form a larger pattern in the later nineteenth century: according to Huxley, the turning-point was the publication of the **Origin of Species** in 1859. The success of the clergy in suppressing heterodox views owed much to the general support that they could rely on among the propertied classes for their efforts. This, in turn, depended upon the political climate in Britain between 1815 and c.1849. During these years, proletarian discontent seemed to threaten revolution; infidelity and politically dangerous ideas appeared to go together, while Christianity posed as a bulwark of the social order. After 1850, however, these conditions changed: the threat of insurrection receded and the Church itself became the target of widespread criticism.<sup>26</sup>

Conditions were favourable for an attack upon the intellectual as well as on the social and

political pretensions of the clergy. 'We are', Huxley announced portentously,

in the midst of a gigantic movement greater than that which preceded and produced the reformation, and really only the continuation of that movement. But there is nothing new in the ideas which lie at the bottom of the movement, nor is any reconciliation possible between free thought and traditional authority. One or other will have to succumb after a struggle of unknown duration, which will have as side issues vast political and social troubles. I have no ..... doubt that free thought will win in the long run ..... or that this free thought will organise itself into a coherent system, embracing human life and the world as one harmonious whole.<sup>27</sup>

'Free thought', in the case of science, meant freedom from clerical interference and freedom to proceed into all areas of speculation that might be designated 'scientific'. Liberation led smoothly into conquest; after a generation of acquiescence to the restrictions that had been put on their activities, British scientists in the mid-nineteenth century (apart from a 'narrow circle of the peace at-any-price "reconcilers" ') began to break down these fences and even to venture into areas of the greatest theological sensitivity. A 'vast tract' of intellectual territory, formerly belonging to the 'old world', was, Huxley recalled, thereby 'steadily invaded and annexed by the citizens of the new world' of science.<sup>28</sup>

A naturalistic conception of 'human life and the world as a whole' was at once the means and the end of this invasion. Naturalism involved a denial of all those absolute boundaries between God and nature, spirit and matter, mechanism and teleology, with which theologians had hedged about cosmological speculation. The articulation of such a doctrine was therefore itself a challenge to the authority which the clergy claimed to have over these matters. Moreover, the naturalist cosmology embodied, according to its apologists, the principles implicit in the scientific enterprise itself; when these principles became explicit and unchallenged, then science would come into its own.

The naturalist movement was overtly anti-clerical; however, it found qualified support within the Church itself. This unholy alliance is indicative of the wider context within which the professionalisation of science occurred. At much the same time, roughly between 1850 and 1880, a faction developed within the Anglican clergy which sympathised with the goals to which naturalism

was directed; in fact, it may be said to have represented the converse of the same process. An anonymous article in *The Reader* (a journal published by Huxley, Tyndall and Spencer) in 1864 provided a survey of the factions active in the Church of England: among the clergy there were

three parties known as the High Church, the Low Church and, the Broad Church, or, according to another mode of distinction more witty than reverent, as the Attitudinarians, the Plitudinarians, and the Latitudinarians.<sup>29</sup>

The last group was small, but had attracted attention disproportionate to its size by the ideas its members propagated. These were

not included in the inventory of the notions either of the High Church party or the Low Church party, but having their origin in the intellectual ferment of the age in which we now live — ideas partly resulting from the fertile labours of recent historical criticism and research, partly from the necessary modifying action of scientific discoveries and speculations upon philosophy proper — such ideas have entered the minds of some of the ablest and noblest men among the English clergy, and had there become organic and vital.<sup>30</sup>

It was to these 'Liberals' that John Tyndall referred when he confessed an ambivalence towards theologians: some of them he could 'hew to pieces before the Lord in Gilgad', while others he found 'gentle and noble'. The 'problem of the future', he told Huxley, 'will be to detach the one from the other'.<sup>31</sup>

This separation was made easier by the fact that the freethinkers in the Church had much in common with the scientific naturalists — the same enemies for example. The furore in the clerical world that followed the publication of the *Origin of Species* was quickly replaced by a still greater controversy over *Essays and Reviews*, the manifesto of the liberal theologians. Among the most outspoken critics of the latter work was the Bishop of Oxford, the same Samuel Wilberforce with whom Huxley had contended at the meeting of the British Association for the Advancement of Science in 1860.<sup>32</sup>

Liberal Churchmen, like Benjamin Jowett in Oxford and Charles Kingsley in Cambridge, were able to negotiate successfully with the spokesmen of scientific naturalism. Huxley was quite prepared to countenance the survival of the Anglican Church if it were led by their kind; he told Kingsley in

1860 that

it is clear to me that if that great and powerful instrument for good or evil, the Church of England, is to be saved from being shivered into fragments by the advancing tide of science — an event which I should be very sorry to witness, but which will inevitably occur if men like Samuel [Wilberforce] of Oxford are to have the guidance of her destinies — it must be by the efforts of men who, like yourself, see your way to the combination of the practices of the Church with the spirit of science.

In return Huxley demanded that Kingsley and his friends should concede that the 'new school of prophets' — the scientific profession — was 'the only one that can constantly appeal to nature for evidence that it is right'. In other words, the clerics should accept that nature was outside their domain and that it was the preserve of another professional group; they should also acknowledge that it was futile to 'try to barricade us with shovel hats and aprons, or to talk about our doctrines being "shocking"'.<sup>33</sup>

The cooperation between liberal theologians and the naturalists is explicable on the hypothesis that both groups were different wings of the same movement. Each strove to demarcate a field of cultural activity; in particular, both shared an interest in seeing the universities transformed into institutions where professional intellectuals could find employment, whether in science or in theology faculties. 'Liberal theology' was the characteristic rhetoric of the forerunners of a new type of theologian, just as naturalism was that of professional science. The two factions could unite to overcome the opposition of entrenched interests to the transmutation in the social role of educational institutions which the new professional elites demanded.

### iii. The Struggle for Resources

The professionalisation of science involved, above all else, the creation of jobs. Science could not be the livelihood of a group of full-time workers until an institutional structure through which their careers could proceed had been established. By 1850 there were the rudiments of such a structure in Britain; jobs did exist for scientists in industry, government and education.<sup>34</sup> However, in the succeeding decades there was growing evidence that demand for scientific employment was outstripping supply. If professionalisation were to proceed on a scale comparable to that in France and Germany, the inhibitions on further growth inherent in British society had to be overcome and an adequate set of institutions founded. This goal was pursued chiefly by those who had, usually with great difficulty, made a career for themselves in science under existing conditions; these formed the vanguard of a potentially much larger scientific profession.<sup>35</sup>

Huxley epitomised the unsatisfactory state of science in England, which 'does everything but pay. You may earn praise but not pudding.'<sup>36</sup> He had reason from his own experience for this complaint; his career exemplified many of structural weaknesses of British science in the first half of the nineteenth century. The son of a schoolmaster, Huxley had wished to become an engineer, but, because of family contacts, settled for medicine. He served an apprenticeship in London, under the unreformed system of medical education, and then matriculated at London University in 1842; he graduated M.B. in 1845. He acquired his basic knowledge of biology incidentally while working at Charing Cross Hospital: there was no formal instruction in life science available independently of medicine.

Huxley undertook the microscopical researches upon marine fauna that made his name while surgeon on H.M.S. Rattlesnake during a voyage to the Pacific. On the strength of these efforts he was elected FRS in 1851, and, when he returned to Britain, decided upon a career in science. However, it was three years before he secured an appointment. The difficulties that he experienced during this period led him to make some mordant comments on the provision of employment for scientists in Britain. 'To attempt to live by any scientific pursuit', he declared in 1851,

is a farce. Nothing but what is absolutely practical will go down in England. A man of science may earn great distinction, but not bread. He will get invitations to all sorts of

dinners and conversazioni, but not enough income to pay cab fare. A man of science in these days is like an Esau who sells his birthright for a mess of pottage.<sup>37</sup>

The position was especially dire in Huxley's chosen field 'physiology', a term which in the mid-nineteenth century was roughly equivalent to the modern 'biology'. Shortly after his election to the Royal Society, Huxley commented that of one thing

my opportunities for seeing the scientific world in England force upon me every day a stronger and stronger conviction. It is that there is no chance of living by science. I have been loth to believe it, but it is so. There are not more than four or five offices in London which a Zoologist or Comparative Anatomist can hold and live by. Owen, who has a European reputation, second only to that of Cuvier, gets as Hunterian Professor £300 a year! which is less than the salary of many a bank clerk. My friend Forbes, who is a highly distinguished and a very able man, gets the same from his office of Palaentologist to the Geological Survey of Great Britain.<sup>38</sup>

While a literary man could at least support himself by writing for magazines and reviews, even this was closed to the scientist. 'I could get anything I write into any of the journals or any of the Transactions, but I know of no means of thereby earning five shillings'. Huxley concluded that a man who chooses a life of science chooses not a life of poverty, but, so far as I can see, a life of **nothing**, and the art of living on nothing has yet to be discovered.<sup>39</sup>

When in 1854 Huxley did eventually find employment, it was as lecturer at the Royal School of Mines. This was primarily an appointment in geology, a subject in which he was obliged to acquire a competence. He was still unable to find work in his chosen field of biology. Such frustrations were not peculiar to Huxley: his career was contemporaneous with that of W.B. Carpenter, who recalled the problems of life science at this time to the Devonshire Commission. Asked whether there was a need to give additional encouragement to biology, Carpenter replied:

I cannot see that there is any encouragement at present for the prosecution of biological science. I can only speak from my own experience, that when I desired as a young man to devote myself to that study, and had given (I think I may say) some evidence of capacity to promote it, I had to consider how it was possible for me to gain a livelihood; and found that

the only mode in which I could obtain the means of devoting myself to science, was by popularizing, as far as I could, certain portions of it in writing and lecturing, that I might obtain a small fraction of time to give to original inquiry; and I do not think that as regards biological science there is much greater opening now.<sup>40</sup>

But while the problems of lack of opportunity were most acute in biology, they were not confined to life science. The career of John Tyndall, another protagonist in the naturalist movement, shows a similar pattern in the physical sciences. The absence of a formal system of scientific education was compounded by the lack of an assured career in his field as well. Tyndall came to science by way of working on the ordinance survey and as a railway engineer. His only formal scientific education in Britain was gained at the Mechanics Institute in Preston where he attended courses for a time. While a teacher at Queenwood College (an Owenite establishment which was one of the first schools in the country to offer science) Tyndall supplemented this learning in his spare time, with the assistance of Edward Frankland another member of staff. In order to proceed further, it was necessary for both men to travel to Germany to attend lectures and laboratories in Marburg; there they obtained PhDs.

Despite this distinction, upon his return to Britain, Tyndall 'looked every way in vain for an appointment'. Eventually, he gave up and returned to Germany, with the help of a wealthy friend; there he met Clausius and Du Bois Reymond as a peer. When in 1851 he again tried to find a job in Britain, Tyndall was able to muster Faraday, Joule, and Bunsen among his referees; nonetheless he was again disappointed. The 'number of Chairs in England and Scotland', his biographers recalled, 'were few indeed compared with to-day'. Both Tyndall and Huxley tried at this time in despair to find work abroad, but their applications in Canada and Australia also failed.<sup>41</sup>

In 1852 Tyndall accepted the position of Professor at the Royal Institute, which brought a salary of £100 a year. This he could supplement by lectures and translations, which yielded a further £80. This gave him a total annual income of £180: a little more than half of Owen's £300 which Huxley had compared unfavourably with the earnings of a bank clerk. He was 'even worse off financially than at Queenwood', and his position did not greatly improve until he became Professor of Physics at the School of Mines in 1859.<sup>42</sup>

From the outset of their careers, Huxley and Tyndall were accepted as outstanding workers in

their respective fields; their fates consequently represent the **best** for which aspirant scientists could hope in the 1850s. Lesser figures did not even achieve such success; in biology especially medical practice remained an indispensable support for many workers. The institutional space was simply too cramped for all those intent on a scientific career to find room. From this decade onwards, however, the originally disjointed protests against the excessive 'individualism' of British science — at the lack of large-scale scientific establishments — became a coordinated polemic.

Charles Babbage sounded the key-note of this movement in 1851, when he declared that science in England is not a profession: its cultivators are scarcely recognised even as a class.<sup>43</sup>

Offices of 'a strictly scientific nature', he insisted were few, 'and their salaries of small amount'. Apart from the small number of chairs in the Universities, there were only the posts of Astronomer Royal; the Mastership of Mechanics to the Queen; the job of Conductor of the National Almanac; the Directorship of the Museum of Economical Geological and Geological Survey; and a variety of lesser offices. On the biological side, only the Natural History Department of the British Museum offered employment to scientists. The most valuable of the offices, that of Astronomer Royal paid £1,300 annually; when this was compared, Babbage argued, with

the most successful prizes in the army, the navy, the church, or the bar, it shows at once the inferior position occupied by science.<sup>44</sup>

In the course of the next decade, such arguments appeared regularly in the output of a tightly-knit group of polemicists. These founded their own organ **The Reader** in 1863, the forerunner of **Nature**, as a means of making their demands known. An anonymous article in 1865 (probably by Huxley) decried the state of science in Britain, claiming that physiology in particular had suffered because of the action of 'some extrinsic agencies working disastrously upon scientific culture in England'. Chief among these was

the small social consideration paid to a scientific career. Most people, of course regard science as a hobby, which a man may take up who has time on his hands, and does not care for shooting, fishing, or society.<sup>45</sup>

Science was seen as akin to poetry: it was a task for inspired individuals. However, the 'true relations



of the man of science are far closer to the man of business than to the poet or *litterateur*.' What was required was a social organisation for science comparable to that of industry in that it provided not only for a few 'great men', but for the employment of large numbers of workers:

Division of labour is as important in science ..... as in industry; and there is room in science for the workman and the middleman to secure honourable places, though they fail to reach the seat of genius.<sup>46</sup>

Benjamin Brodie made the same point even more strongly in **Nature**: British science was too atomistic to supply the needs of all those who sought employment in it. There were in Britain a limited number of individuals of powerful intellect and elevated aspirations, who have made scientific research the main purpose of their lives ..... The labours, however, of modern science are on far too extensive a scale to be carried on simply by the efforts of eminent individuals. Science requires the services of a class devoted to the extension of knowledge, precisely as other classes of society are devoted to commerce, to politics, or to agriculture. Such a class does not exist among us, and its absence is the greatest defect in our social system.<sup>47</sup>

The remedy of this defect required resources. Wealth had to be diverted to provide the salaries, laboratories and other facilities that a class dedicated to science needed. The politics of scientific professionalisation were directed, therefore, to the identification and occupation of areas where such resources might be found. There were three major possibilities: industry, government and education. Science had a base in each of these that could be extended; however,

in all three of these contexts of the institutional structure of Victorian Britain, countervailing forces inhibited the unconditional endorsement of science necessary for the achievement of full professional status.<sup>48</sup>

In the case of industry, demand was restricted to chemists and, to a much lesser extent, physicists. This demand did grow during the nineteenth century, but was curtailed by the structure of British industry itself. This was still largely a relic of the first industrial revolution, and as a result based upon small units and low technology. The capital to finance extensive research was lacking, as was any strong sense of the economic value of such activity: the 'return on abstract research was simply too low to

excite the continuing large-scale investment demanded of any manufacturer, at a time when British products enjoyed an effortless command of world-markets.<sup>49</sup> By the later 1870s, this complacency was somewhat shaken by foreign, especially German, competition whose success was founded on a systematic exploitation of the potential for industrial innovation inherent in research. Then British manufacturers did begin to show a greater interest in science, but this tended to be too little too late.

In mid-Victorian Britain several state agencies existed which gave some support to science; they covered a variety of areas in which the utility of science to some aspect of government activity had been recognised. For instance, the Admiralty maintained observatories at Greenwich and at the Cape of Good Hope, because of their value in supplying navigational information. The Office of Works ran the Ordnance Survey of Great Britain, as well as the Botanical Gardens at Kew, Edinburgh and Dublin. The Board of Trade maintained the Exchequer Standards Office and, with the help of a committee of the Royal Society, the Meteorological Office. A collection of other bodies, including the Geological Survey, were managed by the Privy Council. The British Museum, although a public institution, was controlled by a Board of Trustees.<sup>50</sup>

There was room for growth in all of these fields. A. Strange in his evidence to the Devonshire Commission went to great lengths to point out inadequacies in the current state-sponsored agencies and to stress the need for extra resources. He argued that in addition to the above bodies, the state should provide an observatory for astro-physics; one for the study of terrestrial phenomena such as magnetism and meteorology; and laboratories for metallurgy and physiology. The creation of such organisations would enhance government employment of scientists: in Strange's words,

if there were any large group of public institutions for scientific purposes conducted by the State, the necessity would arise for such a staff of trained attendants and operators of different kinds.<sup>51</sup>

Moreover, much wider vistas of possible employment in government were opening up for scientists in mid-Victorian Britain. These were the years of growing 'social awareness' and of attempts to alleviate through legislation and state action some of the consequences of uncontrolled industrialisation and rapid urban growth. Scientists could pose as the 'experts' best qualified to

advice on these matters. Examples of such specialist bureaucracies already existed in the 'medical police' of the German states which coordinated public health policy.

There were, however, political obstacles to the increase in public spending that all such schemes would involve. An electorate which, even after 1867, retained a strongly middle class character was usually hostile to any expansion in government activity that necessitated a rise in taxation. The two political parties respected this prejudice. The Liberals in particular remained wedded to a rhetoric of minimal government. Tory paternalism was apparently more amenable to an active government policy towards poverty and disease. But, in practice, neither party did more than develop ad hoc expedients to deal with especially troublesome aspects of the 'social problem'. Only in the 1880s, with the rise of social imperialism and other collectivist movements did a significant political movement committed to interventionist government appear.<sup>52</sup>

In consequence, the opportunities for science to expand in government, as in industry, were limited. Although these areas were not ignored, the main attention of the professionalisers was consequently focussed upon securing a place for science in education. In part, they were guided in this choice by the example of Germany, to which the members of the movement were fond of comparing Britain in unfavourable terms. In the former country science had been professionalised chiefly through the educational system in which it had come to occupy an important status at all levels. As a result, in Germany, Carpenter plaintively remarked, there was no need for a young man eager to pursue a scientific career to 'wait long for the means of obtaining a living.'<sup>53</sup>

In contrast, the British educational system scarcely noticed science. In the primary schools, Huxley wrote, children were taught to read and write, plus a 'quantity of dogmatic theology', and a 'good deal of Jewish history and Syrian geography' — but no science.<sup>54</sup> The neglect of science in the secondary schools was a theme to which the witnesses at the Clarendon Commission frequently returned. Lyell, for example, when asked his opinion of the position of the sciences at this level of education, replied 'I think it is hardly too strong a term to say that they have been ignored.'<sup>55</sup>

It was the state of science at the tertiary level which caused most comment. In the English universities, science had been taught in the London colleges since the early nineteenth century, but

chiefly in conjunction with medicine. At Oxford science was represented by four chairs at the beginning of the Victorian period, and at Cambridge by eight; these numbers rose steadily in the succeeding decades — largely in response to external pressures. In neither of the ancient universities, however, was science an integral part of the system of instruction: the stress was instead upon the concept of a 'liberal education', which,

whilst the emphasis might vary from classics at Oxford, to mathematics at Cambridge or philosophy at Edinburgh — in no way required science as a fundamental component, let alone encompassed the possibility of a degree based solely on scientific knowledge.<sup>56</sup>

Thus in Cambridge, although a Natural Sciences Tripos was established in 1848, it did not itself lead to Honours; it was necessary to fulfil the requirements for an ordinary degree in another subject before taking up science. So long as the new Tripos did not lead to Honours, it had little appeal to students: between 1848 and 1855 only 43 candidates took the Natural Science Tripos, and only in 1875 did the Tripos list include more than 20 names.<sup>57</sup>

It was this indifference of Oxford and Cambridge to science that provoked the most vehement condemnations. In Brodie's words, 'the utter apathy in regard to the advancement of knowledge which has so long prevailed at the English Universities ....., without any doubt, is the main cause of our disasters.'<sup>58</sup> Henry Roscoe, similarly declared that the most important distinction between English and German science was that, while in the latter nation the universities devoted vast resources to the subject, 'our richly endowed Universities ..... have as yet failed to play the important part in this essential feature of modern education which, from their position and means, we have a right to expect them to do.'<sup>59</sup>

Oxford and Cambridge had such importance in the strategy of the movement for scientific expansion for two reasons. The first was that the ancient universities were seen as the crucial redoubt which the reformers must capture if they were to gain access to the educational system as a whole; as long as they held out, efforts to remove the bias against science in the schools would fail. Lyell held the reason why the public schools neglected science was that 'being preparatory, in a great measure, to the universities, they frame their system in regard to those subjects which are to obtain the chief rewards, prizes, and honours at the universities.' The result was that

whatever be the plan adopted at the universities, and particularly whatever may be the matriculation, the entrance examination to the university, that will in no small degree govern what is taught in public schools if any branch of knowledge is entirely omitted.

This was the pattern in respect of science in the old public schools, and where they led the newer seats of secondary education followed.<sup>60</sup>

Huxley echoed this view at the 1866 meeting of the BAAS. The lack of proper science education in the schools was, he argued, 'the fault of the universities, for just as in the story, "Stick won't beat dog, dog won't bite pig, and so the old woman can't get home," science would not be taught in the schools until it was recognised by the universities.'<sup>61</sup> In response to Huxley's arguments a committee was set up to consider means of promoting scientific education in the schools; it reported that 'without the cooperation of the universities, Science can never be effectively introduced into School education.' Although only 35% of the pupils even at the established public schools proceeded to university, 'the curriculum of a public school course is almost exclusively prepared with reference to the requirements of the Universities'. There could be no more decisive proof that 'the Universities and the Colleges have it in their power to alter and improve the whole higher education of England.'<sup>62</sup>

The other reason that the ancient universities attracted so much attention was their wealth. At a time when science was desperately poor and new money difficult to raise because of government parsimony, Oxford and Cambridge were depositories of vast endowments which could be turned to scientific purposes. It was the Colleges, rather than the Universities, which controlled most of these resources. In 1871 the total income of Oxford University was £47,588, and that of Cambridge £34,049. In themselves these were considerable sums, but they were dwarfed by collegiate riches. The income of the combined Oxford Colleges in the same year was £322,680, and a further £35,417 came in from trusts. At Cambridge the corresponding figures were £278,970 and £27,540.<sup>63</sup>

Most of the Colleges' spending was on the endowment of fellowships: in 1871 Oxford disbursed £131,147 for this purpose and Cambridge £123,121. The second largest call on collegiate funds were scholarships and exhibitions to which £25,514 and £24,308 were devoted.<sup>64</sup>

The greatest concentration of wealth available for education in Britain was thus expended in a way which gave minimal benefit to science. The sciences suffered, firstly from the imbalance between

the Universities and the Colleges. In both Germany and in the United States a profession of academic scientists had been established through the creation of central departments in the universities to which chairs and lectureships were attached; these departments included the laboratories and museums that were essential to scientific teaching and research. In contrast to this distribution of resources, the most important unit at Oxford and Cambridge was not the University department but the College. In 1871, Oxford spent £4,648 on Professors and £1,916 on 'Scientific institutions', and Cambridge very similar sums. The Colleges did subsidise the Universities in this respect, but only to a small extent: the Oxford Colleges provided £6,649 for Professorial salaries while spending £18,088 on College servants; their Cambridge counterparts donated only £1,071 for the former purpose and £12,900 for the latter.<sup>65</sup>

Nor did the Colleges remedy this chronic shortage of funds for science in their distribution of fellowships and other prizes. The 1867 BAAS committee on scientific education noted that although it was possible to study science at Oxford, 'At present only a few of the Colleges have lecturers on this subject; while for classics and maths every College professes to have an adequate staff of teachers'. Moreover, with the exception of Magdalen, 'no College has hitherto assigned any scholarships in natural science'; in consequence, of some 600 scholarships and exhibitions available, only one was offered specifically to students of science.<sup>66</sup> At Cambridge, there were 'only five Colleges ..... that take any notice of Natural Science'. King's offered two exhibitions in science, but had no lecturers; Caius had a lecturer in medicine, and offered one scholarship; Sidney Sussex provided two scholarships and a laboratory for students; St John's a chemistry lecturer and a laboratory; and Downing a joint lecturer in medicine and natural science. Only the last two Colleges considered scientist when awarding fellowships.<sup>67</sup>

Oxford and Cambridge were not orientated, therefore, towards the needs of the emerging scientific profession, nor indeed to those of the developing academic profession as a whole in Victorian Britain. Instead their resources were organised and distributed almost exclusively in accordance with the interests of another group: the clergy of the Anglican Church. As Babbage sourly remarked, 'All established religions are, and must be in practice, political engines — they have a strong tendency to self-aggrandisement. Our own is by no means exempt from this very natural infirmity'. This tendency has been most evident in the universities where the Church had appropriated 'to itself all those

professorships ..... which are connected with science'; as a result, 'the larger portion of these ill-remunerated offices had been filled by clergymen.'<sup>68</sup> Moreover, the much greater Collegiate resources had also been turned over to ecclesiastical purposes.

The universities were to a large extent seminaries of the Established Church in the early and mid-Victorian periods. In the first half of the nineteenth century, 32.6% of Oxford students and 23.3% of Cambridge went into the Church. More importantly, the College fellowships were distributed chiefly among clergymen, not for educational purposes, but as part of Church patronage. In short, the 'two ancient universities were ..... an integral part of the Anglican Establishment'.<sup>69</sup>

It was the existence of this clerical interest which was the chief obstacle to the diversion of funds from collegiate to central uses. Attempts had been made to remedy the relative poverty of the Universities; the Oxford Commission of 1852 - 3, for example, was in favour of 'measures calculated greatly to raise the importance of the professorial body so that we may hope to see its ranks filled with able and active men in all departments'.<sup>70</sup> In effect, they advocated an increase in the number of secular academic roles in the universities, a reform which would have particularly benefited science. But the clerical tutors in the Colleges 'saw in this proposal a deadly blow to the cherished belief that education was the province of the clergy. Many of the professors, perhaps a majority would be laymen.' In the words of the tutors response to the Commissioners, 'The effect and indeed the avowed object of this recommendation is largely to remove education from the hands of the clergy.'<sup>71</sup>

The effect of clerical opposition was to thwart the most radical of the Commission's proposals, and, therefore, to baulk the interests of science. Little had been achieved by this exercise in reform, Brodie complained;

A few professorships of ancient date founded by men of very different stamp, which the colleges had suppressed, were revived, but no real or adequate provision was made even for the maintenance of lecturers or professors necessary to carry out the education of the place, and out of these vast funds, not a sixpence was devoted to the advancement of knowledge or the promotion of scientific or literary research, or to the support of museums or laboratories.<sup>72</sup>

Nor was the educational power of the Church confined to the universities. In the early-Victorian

period the Church had reasserted its traditional role as the nation's educator and, by 1868, had achieved a considerable revival in the field. Not only were Oxford and Cambridge 'Anglican preserves', but 'secondary education was given mainly by "public" and endowed schools usually directed by clergymen, and in the last fifty years the Church had increased its hold on education by building and running three-quarters of the primary schools in the country.'<sup>73</sup>

In these areas of education too the Church had used its influence to restrict the growth of science. In the 1850s, a movement had developed to introduce science teaching into elementary education; by this time, many of the conditions necessary to its success were satisfied: there was a supply of teachers and an administrative structure had been devised to support the move. 'Events seemed poised for a significant advance.'<sup>74</sup> But this potential was not realised, because the achievement of science teaching in the schools was dependent upon the goodwill of the Anglican Church which, after 1855, was not forthcoming. Under the regime that controlled Anglican educational policy in the later 1850s and 1860s, 'science was assigned a lowly place in the scale of subject values'; the movement for reform was halted.<sup>75</sup>

The vicious circle was complete. By its control of the universities the Church effectively monopolised the resources that could fund a scientific professoriate; by its control of school curricula it cut off the flow of scientifically trained students moving into higher education which would justify the existence of such a professoriate; moreover, the ecclesiastic hold on teaching posts in both primary and secondary schools deprived science of another possible area of employment. These facts, compounded by the other grievances discussed above, account for the anti-clericalism evident among certain influential scientists from the 1840s onwards. Lyell wrote in 1844 that

One great evil which I complain of in our system of education, both in schools and colleges, is the monopoly of all professorial and tutorial places, masterships and usherships in schools, etc, by the clergy.

The 'Catholic' propensities of clerics meant that, not content with chairs in divinity, church history and Hebrew, they had seized 'upon professorships of astronomy (Armagh), geology (Cambridge and Oxford), botany (Cambridge), mineralogy (Oxford and Cambridge), natural philosophy (King's College, London), engineering (ditto), political economy (ditto .....), and I could give you a long list of others.'<sup>76</sup>



Lyell incorporated these criticisms into his *Travels in America* (1845). The American universities and schools were represented as amenable to science **because** they were free from clerical influence; the contrast with Britain was obvious: 'how unpropitious to the cultivation of physical science is the ecclesiastical spirit, whenever it obtains an undue power in academic institutions'. In Oxford, for example, many College tutors were 'opposed to the cultivation of physical sciences, on account of their irreligious tendencies'.<sup>77</sup> Darwin welcomed Lyell's book recognising that

in your university chapter, the clergy and not the state of Education are most severely and justly handled; and this I think is very bold for I conceive you might crush a leaden-headed old Don, as a Don, with more safety, than touch a finger of that corporate animal, the clergy. What a contrast in education does England show itself.<sup>78</sup>

More generally, *Travels in America* was widely 'cited as authority in pamphlets, there being a stir for reform in higher as well as in lower depths.' Public opinion, Lyell held, was 'rapidly strengthening. There is a move now in the right direction'. He was sufficiently realistic, however, to recognise that 'the clerical influence arrayed against all progressive sciences, whether physical or literary, is too powerful to be easily overcome.'<sup>79</sup>

The resilience of clerical influence was demonstrated when, fifteen years later, Lyell repeated the same complaints to Huxley. Why, he asked rhetorically, should education and its influence on society be left 'exclusively to those 60,000 sworn teachers of endowed opinions'?<sup>80</sup> Later in the same year he told Huxley of a book which showed that the universities 'came in **modern** not in monastic times to fall, under ecclesiastical domination and to throw off the wholesome domination of lay professions, legal, medical and scientific, which would otherwise have governed them and as a consequence a large share at least of our public schools.' Because of this continued domination, current reforms would, Lyell predicted, be ineffective:

Progressive sciences will never and cannot have fair play since the college tutors ..... will always be nearly all parsons in ..... future.<sup>81</sup>

At the same time, however, opposition to this state of affairs was growing. In the course of the mid-Victorian period a 'new' professional middle class developed to challenge the power of the old.<sup>82</sup> This process was evident in the universities themselves. Although in the early nineteenth century a don

was 'by profession a clergyman, not a university teacher', by the 1830s this view of the functions of College fellows was challenged by an alternative concept of the academic as a distinct professional, independent of the Church, who was devoted to teaching and research. This role was 'tied closely to the idea of the professorial system as it existed on the Continent and particularly in Germany.'<sup>83</sup>

Despite the opposition of the old guard, a fifth column of the new-style professional academics was established at Oxford and Cambridge by the 1860s. Among these were a few scientists whose numbers grew painfully slowly during the next decade as the Colleges made fellowships available to science in a piecemeal way. However, further growth, as Mark Pattison realised, could not rely on such 'improvements in detail'; what was required was 'nothing less than a change in the aims and objects' of the ancient universities.<sup>84</sup> He called for a revolution in the institutions of education comparable to, and in line with, those which had occurred in other areas of British society.

Pattison recognised the difficulties in the way of this strategy: 'it is still only a small minority, and chiefly of academical and scientific men, who yet see their way through the difficult problem of university reform. The conception of an organised profession for the preservation and transmission of knowledge has not yet become familiar to the English mind.' Nonetheless, he argued that the time had come for a transformation of the universities to fit the general shift in British social structure: the middle class now demanded education, and a profession was needed to service this need. But, Pattison insisted, this involved a reallocation of Collegiate funds; 'the profession of learning will not exist at all, as a profession, with regular succession, unless it is created by endowment. There is no other fund but this reserved portion of the national domain, seeking to employ the practitioners of such a profession.'<sup>85</sup>

E. Ray Lankester made the same point from the specific standpoint of science in the universities. He was one of the scientists who had broken into the clerical preserves of Oxford as a fellow of Exeter College. However he claimed in 1872 that the professoriate in Oxford was still an effete body, while the 'Colleges are really, without any exaggeration, now nothing more than large proprietary schools'; the entire institution remained 'half-choked by ecclesiastical and aristocratic odours'. In consequence, Lankester's activity was restricted to cramming students for exams, and this arrangement prevented him from 'developing any prowess as a teacher — and keeps one back from such research as is

always suggested in an actively worked laboratory.' In fact, the experimental facilities available to him were confined to 'a little room fitted with a microscope work room'. Lankester concluded that the only chance of getting satisfactory pupils — or of having adequate apparatus to work with — or a proper sub-division of subjects among teachers — is ..... for the central institution — the Museum — to be strengthened by funds from the Colleges.<sup>86</sup>

When, in the same year, the government responded to pressure from the BAAS and elsewhere and set up a Commission to examine the endowments of Oxford and Cambridge, the reformers mobilised to ensure that a significant proportion of these funds was assigned to the maintenance of an academic profession.<sup>87</sup> A meeting was held in Oxford in 1872 to establish a Society for Academical Study, and to resolve that 'to have a class of men whose lives are devoted to research is a desirable national object'. Specifically, the society demanded that 'professorships and special institutions shall be founded in the Universities for the promotion of scientific research', and declared that 'the present mode of awarding fellowships as prizes has been found unsuccessful as a means of promoting mature study and original research, and it is desirable that it should be discontinued.' In place of the existing distribution of resources in favour of the clergy,

a sufficient and properly organised body of resident teachers of various grades should be provided from the Fellowship Fund.<sup>88</sup>

Present at this meeting were John Seeley and Henry Sidgwick, as well as Brodie, Carpenter and Huxley. These represented the coalition behind the reform movement; not **only** the interests of science were at stake. It was desirable that 'there should be adequate provision for men who desire to devote their lives to research' in philology, archaeology and other disciplines in addition to the natural sciences.<sup>89</sup> The reorganisation of university finance and the transmutation of the role of fellow from that of cleric to that of full-time teacher and researcher accorded with the interests of the academic profession as a whole.

But while such a transfer of resources suited the aims of one social group, it involved the despoilment of another. The intended victim of this exercise, the Established Church, was not defenceless, however: its intrinsic strength was augmented by other sections of Victorian society that shared similar concerns. A takeover of the educational institutions was therefore only possible if a

still greater concentration of political power could be exerted against this combination. Neither the new academic profession together, and still less science alone, wielded such power: they were numerically insignificant, poor, and controlled none of the strategic heights of the economy. The only political resource available to them was a polemical one; the traditional role of the professional intellectual was to act ostensibly as spokesman for other interests while covertly furthering his own.<sup>90</sup>

Success therefore depended upon an alliance with a more considerable political force which pursued ends broadly compatible with those of the scientific profession. In the event, the reformers found such an ally in the radical wing of the Liberal Party, from whom they gained support in Parliament and in the press. In return, the radicals took a view of nature which supposedly conduced to the social vision of 'Advanced Thinkers': it is in this context that the broadest political significance of scientific naturalism in Victorian Britain emerged.

#### iv. Naturalism and Enlightenment

The organisation of society upon a new and purely scientific foundation is ..... the only political object much worth fighting for.<sup>91</sup> (T.H. Huxley)

The point of contact between the scientific reformers and the Radicals was an agreement that the connection between the Church and education must be severed, or at least severely curtailed. The reasons why this was in the interests of the former group have been described above; the attempts of the Radicals to achieve the same end need to be related to more general political issues.

The interdependence of specific and wider concerns in Victorian Britain was illustrated when in 1866 the reform party at Cambridge tried to establish a Chair in American History at the university. On the surface, this was part of their campaign to diversify the field of study at Cambridge and to increase the number of professorial positions. Similarly, the opposition to the measure came from old guard and was founded upon the resistance of clerical fellows to any change that threatened the sectarian character of Cambridge. The first tenant of the new chair was to be a Harvard academic, and, as one critic of the scheme pointed out, Harvard University, 'as far as it possesses any form of religion, is distinctly Socinian, or if the Americans prefer the term, Unitarian.' However, Leslie Stephen's account of how the matter was settled reveals that other questions were also at stake:

Directly I went into the Senate House yesterday I saw at a glance that we were done for. The district around Cambridge is generally supplied with parsons from the University, who can be brought up when the Church is in danger ..... The sons of Zeruiah were too many for us ..... Every intelligent man in the place voted for the professorship even Kingsley, who was very energetic about it, though he has been unsound upon America [ie. upon the American Civil War] generally, but when the Church is having its foundations sapped, and that by an American democrat, it would be easier to argue with a herd of swine than British parsons. I am sorry for it, because it shows that in Cambridge the Liberal Party is weaker than I thought, and because it looks, and in fact is a very ungracious proceeding. I only hope the more that we shall be able gradually to get the University out of its connexion with established Church; but we have a piece of work to do first.<sup>92</sup>

In effect, the vote became a demonstration of Cambridge's stance on the recently concluded

conflict between North and South in America. This, in its turn, reflected upon the relative strength of the domestic political parties in the university: the Liberals had sided with the Union, the Tories with the Confederacy. Just as Stephen was both a member of the reform party in Cambridge and a radical, like his friend Henry Fawcett; so the conservative members of the Senate tended also to be Conservative. Ecclesiastical power in the university was used both to block 'progressive' internal measures and to keep Cambridge in the ranks of the 'reactionary' party in British politics generally.

The same correlation between the policies of the clergy and those of the Tories appeared to hold in other fields also. The Radicals themselves saw the Church and the Conservative Party as the two heads of the same monster with which they were grappling, and which was the real obstacle to social progress: the landed aristocracy. The 'too close' alliance between Oxford and Cambridge and the aristocracy was therefore only one aspect of the assembly of interests against which the left wing of the Liberal Party spent its main effort between the 1860s and the 1880s.<sup>93</sup> In the words of John Stuart Mill, for him and for other Radicals, 'the predominance of the aristocratic classes, the noble and the rich, in the English Constitution [was] an evil worth any struggle to get rid of,'<sup>94</sup> and, to them, the Anglican Church was merely one organ of that class.

The educational functions of the Church attracted special criticism because of the supposed power that these gave to the Conservative interest over the hearts and minds of the electorate. The revival of Anglican activity in elementary and secondary education during the Victorian period exacerbated these fears. In 1847 a cleric named Nathaniel Woodard founded a school in New Shoreham which was to be 'the germ of a great scheme for the recovery by the Church of its position as educator of the nation.' Plans were floated for a comprehensive national system of schools run by the clergy, instruction in which would aim to unite pupils with 'the true faith of the Church'. Among the sponsors of this attempt to 'win the people over, or back to the Church' was Lord Salisbury, the future Tory Prime Minister.<sup>95</sup> He also promoted the 1874 Endowed Schools Bill which was 'a blatant attempt to protect the interests of the established church in secondary education'.<sup>96</sup>

The Radicals strove to end this symbiotic relation between the Church and the landed interest during the 1860s and 1870s: they made the abolition of ecclesiastical influence in education a central aspect of their strategy.<sup>97</sup> While the Tories looked to the doctrines of the Church for 'spiritual' support

for their position; the intellectual aspect of the Radical agitation was epitomised in the pages of the **Fortnightly Review**. The views contained there were variously described as 'positivistic', 'materialistic' and 'naturalistic'; more important than names, however, was the strategy in which certain cosmological resources were used. The policies of Advanced Liberalism were represented as the political aspect of a general process of enlightenment which would issue in both a rational view of the world and in a rational social organisation. 'Science' was the guarantor of the former and also lent its weight to the latter end. As John Morley, the editor of the **Fortnightly** during these years, later recalled,

perhaps those were right who fancied they discerned a common drift and scented a subtle connection between speculations on the physical basis of life or the unseen universe and articles of Trade Unions or National Education; for undoubtedly a certain dissent from received theologies had been found in suspiciously close company with new social and political ideas. And it was the Radical Programme, rather than the Unorthodox Theology, which had excited wrath.<sup>98</sup>

Morley, and others who tried to harness cosmological ideas to political programmes, drew consciously upon a long tradition in which naturalism and radicalism had been united. They represented their efforts as the culmination of a political and intellectual movement that could be traced, through Comte, to the *Ideologues* and thence to the authors of the *Encyclopédie*. The essential feature of the philosophy of the Enlightenment, Morley argued, was its aspiration to **totality**: 'it was animated at once by the scientific and the social idea ..... It rested on a conception of life as a whole. Morality, positive law, social order, economics, the nature and limits of human knowledge, the constitution of the physical universe' were combined in an interdependent whole.<sup>99</sup>

There were more immediate exemplars of this kind of strategy. Naturalistic formulations had been part of the polemic of working-class radicals like Richard Carlile in the first half of the nineteenth century. These retained an 'Enlightenment faith in science as the nihilist of superstition and idolatry and as the herald of a republic founded on natural rights and freedom'. In the London of the 1820s, as in the Paris of the 1790s, the main targets of their attacks were those 'most pernicious threats to human liberty: the aristocracy and organized religion'.<sup>100</sup> Carlile declared in 1822 that it was 'the Man of Science who is alone capable of making war upon the Priest, so as to silence him effectually ..... Why

does he search in Nature and her laws, but to benefit himself and his fellow man by his discoveries, by the explosion of erroneous ideas, and by the establishment of correct principles?'<sup>101</sup>

In the early decades of the century middle class radicals were also active in condemning the 'old corruption' and the priestly and aristocratic elites that presided over it. They therefore had much in common with Carlyle, and also used a naturalistic rhetoric, though with more emphasis upon epistemological than upon cosmological questions.<sup>102</sup> In the course of the 1830s, the social and political meaning of naturalism became increasingly that of a middle class movement; it became part of a rhetoric of **reform** rather than revolution, and firmly wedded to the dogma that 'progress', or gradual improvement, was the natural form of change.

Henry Buckle wrote the history of England around the theme that 'science' was the constant intellectual companion of social and political improvement and 'religion' their chronic foe. The great obstacle to progress in Britain had been 'a spiritual tyranny and a territorial tyranny: the tyranny of the church and the tyranny of the nobles'. These had jealously guarded superstition as a bulwark of their position, but, from the seventeenth century onwards, a process of intellectual innovation had been accompanied by one of social reform: the 'old principles of tradition, of authority, and of dogma, were gradually becoming weaker; and of course, in the same proportion, there was diminished the influence of the classes by whom those principles were upheld.'<sup>103</sup>

Science had been the main agent of this dissolution of the reactionary world view, especially in France; in that nation there had been an 'intimate connexion between scientific progress and social rebellion'.<sup>104</sup> Buckle recognised, however, that the performance of many British scientists had been less impressive in this respect. Many 'even of our most eminent physiologists', for example, 'have shown a marked disposition to ally themselves with the reactionary party; and have not only opposed such novelties as they could not immediately explain, but have degraded their own noble science by making it a handmaid to serve the purposes of natural theology.'<sup>105</sup>

The intimidation that enforced such compliance has been discussed above. Even as Buckle wrote, however, the pressures which the clergy could bring to bear to perpetuate this style of science were diminishing. As a result, the advocates of scientific autonomy were able to promote 'vast and magnificent schemes' which vitiated the dogma of divine interference in nature and replaced it with a concept of the



self-sufficiency of the cosmos. Buckle enumerated the major components of such schemes: uniformitarianism in geology; the theory of the transmutation of species in biology; and the nebular hypothesis in astronomy. In their main features, the world-view of scientific naturalism and the cosmology of enlightenment were identical.<sup>106</sup>

Morley recalled the attractions of naturalism to liberally-minded men in the 1850s and 1860s. There was, in Leslie Stephen's words, 'much empty profession of barren orthodoxy, and, beneath all, a vague disquiet, a breaking up of ancient social and natural bonds, and a blind groping toward some more cosmopolitan creed.' This search for a world-view took place in a climate where the 'temper of vigorous intellectual disputation ..... that had been raised in two spheres so wide apart as the war against the Corn Laws and the war against the Puseyites, had become a sort of mental habit in the country ..... Schools of thought were metamorphosed into combative parties, and, ..... rationalism and natural science blew defiant bugles against the old tradition.'<sup>107</sup>

In this climate, cosmological statements were charged with wider significance: they encoded declarations on ethics, politics and social order. On the one hand, was the party of reaction which included the Church; it justified its position in terms of the sanction given to existing arrangements by a transcendent deity. On the other, was the party of progress, whose outlook was essentially humanistic. While questions about the nature of knowledge or the constitution of the universe might seem abstruse, Morley wrote, and 'may strike the publicist as having the least possible to do with the type of government or the aims of a community. Yet it is really the conclusions to which men come in this region, that determine the quality of the civil sentiment and the significance of political organization.'<sup>108</sup> The particular polemical goals of the Radicals led them to espouse the opinion that Morley attributed to D'Holbach, namely, that

the social deliverance of man depends on his intellectual deliverance, and that the key to his intellectual deliverance is only to be found in the substitution of Naturalism for Theism.<sup>109</sup>

The cooperation and empathy between the radicals and the proponents of scientific professionalism therefore proceeded at several levels. The battle against the forces of reaction involved an assault upon the material foundations of the Church, and this was the major issue in the Parliamentary debates over education in the 1870s. One Conservative MP rightly declared that the University Tests Bill of 1871

was 'one step towards the disestablishment of the Church of England'; it was, said another, 'a measure of confiscation'. While the Tories opposed the Bill Radicals like Fawcett and Charles Dilke combined with the Parliamentary spokesmen of science, Lyon Playfair and John Lubbock, to champion the measure.<sup>110</sup>

At another level, the struggle required the mobilisation of cultural resources to undermine the ideological groundings of the old order, and, in particular, its theology. That the two activities were in reality one was revealed in 1878, when Fawcett proposed that Morley, Huxley, Stephen, Spencer, Dilke and several other Liberal MPs meet 'for purposes of conversation on political and social subjects ..... during the Parliamentary session.' The object of this club was to coordinate the various aspects of the campaign in which all these individuals were in some way involved, and to bring 'into contact the Radical members of the House of Commons, the representatives of the Liberal press, and the leaders of Liberal thought in the Universities and elsewhere.'<sup>111</sup>

The same year saw another attempt to achieve a formal expression of the radical political character of the 'scientific' outlook. W.K. Clifford, a confidant of Stephen and Fawcett, as well as of Huxley and Tyndall, 'had a strong feeling that scientific men were not in sufficient relation with the general intelligence of the country, and not doing enough to liberate the people of all classes from degrading dogmas.' Clifford therefore proposed a Congress of Liberal Thinkers to mark Voltaire's centenary; this was held on 13 and 14 June, 1878 with Huxley as president. In consequence of this meeting, an 'Association of Liberal Thinkers' was formed; its members included Morley, Stephen and Tyndall and it met at Huxley's house. The Association was intended to provide a scientific critique of 'mythology' and a political one of 'the priestly organisation of the churches'; both were necessary because the 'clergy is everywhere making more pronounced its revolt from the great principles which underlie the modern social structure.' Several meeting<sup>s</sup> of the Association were held and new members solicited from among scientists; however Clifford's death robbed the body of most of its impetus.<sup>112</sup>

Plans to introduce more science into school and university curricula could also be represented as serving a grand political plan as well as professional interest. Huxley justified his support for better educational opportunities for women on these grounds. 'I don't see', he wrote, 'how we are to make any permanent advancement when one half of the race is such, as nine-tenth of women are, in mere

ignorant parsonese superstition.' He planned to give his own daughters the same training in physical science as that received by their brother — 'so long as he is a boy'. Unless this practice became general, he warned, 'women will stop at the doll stage of evolution; to be the stronghold of parsondom'. These 'parsons', whose power general training in science would weaken, were, Huxley concluded, 'the drag on civilisation and degradation of every important pursuit in which they mix themselves: intriguers in politics and friponnes in Science ..... If my beak and claws are good for anything they shall be kept from hindering the progress of any scheme I have to do with.'<sup>113</sup>

This awareness of a political role for scientific ideas extended even to Charles Darwin who, despite his resolute withdrawal from the affairs of the world in later life, acknowledged, in private, the wider significance of the naturalism that he helped to develop. Darwin was himself 'an ardent Liberal, and had a very great admiration for John Stuart Mill and Mr Gladstone'.<sup>114</sup> His views on the clergy resembled those of the former more than those of the latter representative of Liberalism: Darwin welcomed Buckle's anti-clericalism, maintaining that 'I do not care whether his views are right or wrong, but I should think they contained much truth.' The admiration was reciprocal; Darwin informed Lyell in 1862 that 'the great Buckle highly approves of my book [*Origin of Species*]'.<sup>115</sup>

Darwin had in the 1840s espoused the radical cause of the slaves in America and had clashed with Lyell upon this point.<sup>116</sup> When the *Origin of Species* appeared in America at the outbreak of the civil war, it was treated as a political document: 'the issues of slavery and evolution were fused'. While Louis Agassiz, maintained the theory that the races of man had been created separately, and wrote an essay supporting slavery; Asa Gray, one of Darwin's strongest American supporters, held that the evolutionary theory when applied to man 'makes the Negro and the Hottentot our blood-relations'; he was against slavery and stood by the North during the war.<sup>117</sup>

A similar association between evolution and 'Advanced' views occurred in Britain during the Eyre controversy. Eyre was a governor of Jamaica who had put down a Negro revolt with great severity, summarily executing several of the rebels. The affair became a rallying-point for the various factions in domestic politics: the Liberals formed a 'Jamaica Committee', chaired by Mill, to demand Eyre's prosecution; Huxley was one of its members. Shortly before, Huxley had published his own work on human evolution, *Man's Place in Nature*, and the *Pall Mall Gazette* in August 1866

queried whether his 'peculiar views on the development of species' had led Huxley to join the Jamaica Committee. Huxley denied this, claiming that his interest in the affair was the result of its implications for home politics and not occasioned by 'any particular love or admiration of the negro'.<sup>118</sup> Nonetheless, the connection was made between a certain scientific orientation and a radical political stance, and this reflected the accepted political meaning of naturalism.

Just as the 'English freethinking of the eighteenth century was in part the offspring of the English Revolution' and the 'French infidelity was one of the movements which prepared the way for the French Revolution',<sup>119</sup> so scientific naturalism in mid-Victorian Britain was an idiom of a radicalism which proposed the critique and reconstruction of existing social institutions. This was not the only political significance of naturalism in the nineteenth century; but from 1860 to about 1885 it was the dominant one. The shift towards a conservative naturalism, which accompanied the seismic disturbance in British politics in the last two decades of the century, is discussed in the final chapter.

## Conclusion

Three clusters of interests, all associated with a particular kind of naturalistic strategy, can be distinguished in Victorian society. Each of them was in some way implicated in the development of a scientific profession during this period, though in some cases a wider set of social circumstances is relevant. The discussion of special instances of these strategies is organised by area; consideration is given to four of the major aspects of cosmology in which naturalistic ideas were used to further one or more of these interests.

In the case of 'Cause', the overtly political element was paramount, although disputes over the nature of causation also expressed controversy over the proper boundaries of scientific and theological knowledge. In the case of 'Life' and 'Mind', the political was more equally complemented by an interest in inter-professional boundary-drawing and in the constitution of scientific specialities. The last interest, in 'internal' definition, was of greatest importance in the debates about the bases of morphology and embryology, but wider concerns were present in this context as in the others.

The period principally covered by this account, roughly 1860 - 1885, includes the heydays of scientific naturalism, of the political radicalism with which it was associated, and the period in which demands for a professional status for science were most vociferous. The final chapter intimates the trends in the social uses of cosmologies in Britain that followed; it pays special attention to conservative strategies, both those which were avowedly anti-naturalistic and those which embodied a new political significance for naturalism.

## CHAPTER TWO: Causes, and Forces, and Powers

Geschrieben steht: Im Anfang war das Wort.

Hier stock' ich schon! Wer hilft mir weiter fort?

Ich kann das Wort so hoch unmöglich schätzen,

Ich muss es anders übersetzen,

Wenn ich vom Geiste recht erleuchtet bin.

Geschrieben steht: Im Anfang war der Sinn.

Bedenke wohl die erste Zeile,

Dass deine Feder nicht übereile!

Ist es der Sinn, der alles wirkt und schafft?

Es sollte stehn: Im Anfang war die Kraft!

Doch, auch indem ich dieses niederschreibe,

Schon warnt mich was, dass ich dabei nicht bleibe.

Mir hilft der Geist! Auf einmal seh' ich Rath,

Und schreibe getrost: Im Anfang war die That!<sup>1</sup>

(Goethe, **Faust**.)

In the beginning of Science, the parsons who managed things then,

Being handy with hammer and chisel, made gods in the likeness of men;

Till Commerce arose, and at length some men of exceptional power

Supplanted both demons and gods by the atoms which last to this hour.

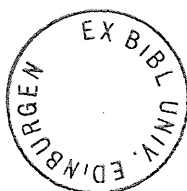
Yet they did not abolish the gods, but they sent them well out of the way,

With the rarest of nectar to drink, and blue fields of nothing to sway.<sup>2</sup>

(James Clerk Maxwell, 'British Association 1874 – Notes on the President's Address'.)

#### i. Spiritualists, Materialists, and Sceptics: the Eighteenth Century Background

At first sight, Isaac Newton exercised an extraordinary influence upon British natural philosophy in the two centuries after his death. His authority was constantly invoked to settle questions in chemistry and biology, as well as in mechanics and physics. However, closer scrutiny reveals that there was not one Newton but several. His name was attached to many divergent programmes and theories, different



of his successors choosing to emphasise different parts of Newton's large and ambivalent output.

This chapter is largely concerned with one of these Newtons. He might be called Samuel Clarke's Newton, since Clarke was the foremost exponent of this version of the master's thought; it is also, incidentally, the character which approximates most closely to the results of recent scholars' search for the historical Newton. This Newton was preoccupied with the theological implications of his cosmology. He was concerned to retain a role for God in nature; he tried to detach atomism from its previous materialistic and atheistic associations and to make it instrumental to this end.

Newton held that matter, in itself, was devoid of active properties: it possessed extension, mass and inertia, but before these could be mobilised to form a cosmos objects had to be acted upon by **force**. Newton tended to associate the concept of force with that of 'active principles'; these were akin to the 'plastic natures' of the Cambridge Platonists and were, in effect, intermediaries between spirit and the world of brute matter. By means of these active principles God's will regulated natural processes: they were, in Newton's own words, the 'manifestation of God's agency in the world'.<sup>3</sup> Spirit was thus the true source of the power and order in the universe; matter remained passive and impotent.

While Clarke and others developed this version of Newtonianism in the eighteenth century, others repudiated it. There was a tendency to erode the categorical distinctions between matter and force and to conceive force as essential to bodies.<sup>4</sup> This took two main forms. Some, who still called themselves 'Newtonians', retained the notion that matter consisted of corpuscles between which forces acted, but treated attraction and repulsion as properties of the atoms. Another group of revisionists distanced themselves more overtly from Newton's emphasis on atoms and forces, and argued that the causes of phenomena inhered in **substances** which composed the universe.<sup>5</sup>

These two conceptions sometimes held common ground in the concept of an ethereal medium which interpenetrated the particles of matter and was the locus of the active fluids. By the late eighteenth century this conception had been elaborated and refined, until electrical, magnetic, thermal and chemical properties were explained in terms of a 'subtle matter' which interpenetrated gross atoms. Young and Fresnel's work in the early nineteenth century was to add the propagation of light to the properties of the ether.<sup>6</sup>

Both these theories tended to reduce the gap between the active and the passive, and hence between

the spiritual and the material upon which Newton had insisted. The 'metaphysical' character of Newton's forces, their evident supra-empirical status and theistic implications, became an argumentative resource for those who wished to abandon these distinctions and to consider nature as a self-sufficient entity.<sup>7</sup> Newton's ontology was also the subject of a more thoroughgoing critique on epistemological rather than cosmological grounds.

The Newtonian world-view was implicitly realist. It assumed the reality of such concepts as matter and force and the objective status of the relations which were posited to exist between them. Causation was conceived as the process whereby force acted upon matter to bring about certain results. From this event, Newtonian divines inferred the existence of another category — an active, powerful God whose action was necessary to explain causation.

This realism was criticised by Berkeley who held that God's existence was best assured on the hypothesis that spirit was the **only** substance in nature. David Hume's sceptical analysis of Newtonian terms received more attention because its author appeared to allow no independent existence to either matter or spirit. In effect, Hume laid bare the assumptions of Newton's cosmology and countered them with a thoroughly empiricist account of such notions as causation.

All knowledge, Hume held, 'degenerates into probability': the human understanding had a strictly limited potential. Man's view of the world depended upon the senses, and the information they supplied was both contingent and fallible; it was only because of the strength of habit, or 'custom', that certainty was attributed to concepts.<sup>8</sup>

For example, Hume held the idea of matter to be 'nothing but collections formed by the mind of the ideas of several distinct sensible qualities, of which objects are composed, and which we find to have a constant union with each other'.<sup>9</sup> Not content with the limits to its knowledge, however, the mind attributed these manifestations to a 'substance or matter' which was supposed to underlie them. Hume maintained that it was impossible to know anything of such a substance; similarly, 'we have no idea of power and agency, separate from the mind, and belonging to causes.' The idea of cause was constituted by constant conjunctions between certain phenomena which, by custom, were designated 'cause' and 'effect'. In this way, Hume repudiated the objective and separate reality of matter and power upon which Newtonian natural philosophy relied; in as far as they had any existence, these were only objects



of the mind with precisely the same status.<sup>10</sup>

Hume went further: he expressly addressed the notion that causation was proof of the action of God in nature. He denied the possibility of conceiving an infinitely powerful being; even if this objection were put aside, the theory that the deity intervened in nature led to absurdity. According to Hume, there was no basic difference between perceptions of the 'external' world and of ideas themselves; therefore, if

we have recourse to him [God] in natural operations, and assert that matter cannot of itself communicate motion, or produce thought .....; I say, upon the very same account, we must acknowledge that the Deity is the author of all our volitions and perceptions.<sup>11</sup>

The true source of 'causal power' was the mind, which imputed the relation of cause and effect to the constant union of two or more objects of experience. 'Cause', therefore, was entirely subjective; further, it was inseparable from the phenomenal relation by which it was established. In consequence, there could be no 'power' or 'force' considered as an entity apart from matter.<sup>12</sup> Such a reduction of the concepts of power and agency left little room for the theological implications that Newton had tried to impress on natural philosophy.

Hume made his dissent from the Newtonian brand of natural philosophy still more clear in his later work. He branded recourse to some 'invisible intelligent principle as the immediate cause' as a primitive superstition which modern philosophers perpetuated. They 'acknowledge mind and intelligence to be, not only the ultimate and original cause of all things, but the immediate and sole cause of every event which appears in nature'. The physical events which were normally designated as 'causes' were, according to these philosophers, merely the 'occasions' of a phenomenon: 'the true and direct principle of every effect is not any power or force in nature, but a volition of the Supreme Being'.<sup>13</sup>

Hume identified these views with those of the followers of Malebranche. It is a token of how quickly Newton's legacy had become a subject of contention that Hume maintained that such opinions were entirely contrary to Newton's own thought. Newton and his followers, Hume alleged, had assigned force to 'second causes' such as the ether; Hume even alleged that this was the position of Samuel Clarke. This was an especially ironic imputation because the form in which Hume stated the

true relation of God to nature was very close to that of Clarke's antagonist, Leibniz. It was more to God's credit if he were assumed to have endowed his creation with the capacity to develop itself among a predetermined path than that the divine will should be necessary to its continued upkeep.<sup>14</sup>

Scepticism of Hume's type was habitually deployed in conjunction with a particular cosmology. Nature was considered as a 'stupendous machine', a self-sufficient entity which might need to be referred to God for its origin, but not for its continuous operation. Empiricism together with this form of immanentism comprised the programme of 'deism'; a programme which, its critics alleged, was nothing less than an assault on the Christian religion. If the supra-empirical were unknowable, upon what basis could dogma rest? If God had endowed nature with full potency, why worship him; was not nature itself a more worthy object of veneration? Deism was but pantheism disguised, they argued, and pantheism the sure road to atheism.<sup>15</sup>

During the nineteenth century the themes outlined above were developed in response to the changing social and political context in Britain. The Newtonian distinctions between matter, force and spirit were revived by one set of thinkers; this move was opposed by new versions of the doctrine that matter was sufficient in itself to form nature. Moreover, immanentism was again closely associated with an empiricism which called for a radical redefinition of the concepts of cause and force. All of these theories were held to have significance not only for natural philosophy, but also for the 'moral' world.

## ii. Nineteenth Century Newtonianism

The breathing of the Divine mind is still thrown upon sensible objects with which it cannot blend, but which it merely sets in motion. Like the pure air, which agitates equally the muddy pool, the clear lake, and the immense ocean, but is ever above them and unmingled with them, it gives form to the waves, but does not change their substance.<sup>16</sup>

(Humphrey Davy)

Deism was, in general, tolerated by all but some Churchmen in eighteenth century Britain; at least, it was an acceptable creed for a gentleman to hold. All this changed with the French Revolution. Then, the clerics who had argued that deism was not merely a threat to the Church but to social order as a whole seemed to be vindicated. The 'Jacobin philosophy of nature' was an extreme version of the deist doctrine; its concomitant social philosophy showed the connection of such ideas with radical politics.

Jacobinism formulated its cosmology as a refutation of the central doctrines of the worldview associated with conservative interests. The most important features of the 'wrong and harmful type of thinking' which the Philosophes attacked were 'belief in God, in spiritual forces of any kind, [and] in a nonmaterial element in man'. Of these, the principal target was the 'dualistic doctrine to the effect that there is an independent thinking substance in addition to extended matter'.<sup>17</sup> The politically correct view, in contrast, was that matter was self-moving and itself the source of all power.<sup>18</sup>

In Britain a similar association between hylozoism and radicalism obtained. Tom Paine, the foremost exponent of revolutionary ideas in Britain, epitomised this link. He represented both 'the infidel movement of England which had spread itself among the lower orders' and the mingling of this movement with the 'political dissatisfaction' of these sections of society.<sup>19</sup> This was a very different matter than the polite aristocratic deism of Gibbon and the relative tolerance of earlier decades was quickly dispelled. In its place appeared attempts at intellectual repression whose victims included unorthodox natural philosophers like Erasmus Darwin and Joseph Priestley.

The chief crime of such thinkers was held to be their espousal of the view that 'human affairs are not subject to the direct intervention of a personal God'.<sup>20</sup> While Darwin denied the action of such an entity in biological and geological phenomena, Priestley was the author of well-known works

in which the distinction between God and nature was denied and mind made indistinguishable from extended matter.<sup>21</sup> In both cases the notion of God the transcendent creator and ruler of the universe was threatened with extinction.

Apart from reaction, there were a number of theological responses to such ideas in the nineteenth century. One of these was that of the High Church party. This simply denied the relevance of physical evidence to religious truths such as the existence of God; on purely intellectual grounds, J. H. Newman held, it was 'a great question whether atheism is not as philosophically consistent with the phenomena of the physical world, taken by themselves, as the doctrine of a creative and governing power.'<sup>22</sup> Religion must rest on faith and on the authority of the Church. The various nonconformist sects similarly stressed the irrational bases of religion, although they placed greater trust in Scriptural than in ecclesiastical authority.

On the other hand, there were parties in British theology who balked at this sharp separation of 'the rational and the moral faculties of the soul' and the consequent diremption between 'philosophical and religious evidence'.<sup>23</sup> Some of these took as their mentor Samuel Coleridge who had imbued nature with action of God: he had supposed a 'Platonic Logos or Son of God; to whom we are to refer at once the physical cosmos, the divine process in history, and the intimations of reason and conscience.' According to this theology, the physical world was a source of information of God's attributes and of his relations to man.<sup>24</sup>

The Newtonian doctrine of matter and force was held to be especially instructive in this respect. These concepts were mediated to the nineteenth century by the Scottish Common Sense philosophy which had been formulated largely as an 'answer' to Hume and to the materialists. In general, the Common Sense philosophers held that human agency provided a crucial analogy in understanding all causation. They had distinguished between 'physical' and efficient' causes; while the former might be described in terms of constant conjunction, the latter had to be referred to the action of a being possessing will and power. In effect, the 'laws of nature were ..... physical causes, dependent upon the unknowable efficient causes which were themselves effects of the divine will'. In this way, the reality of causes was rescued from Hume's scepticism. At the same time the basic contention of Newtonian natural philosophy was secured: 'the ultimate primary cause was God, who actively governed

the world by the exercise of His will.<sup>25</sup>

These views were developed as part of the Coleridgean programme of natural theologians and scientists centred upon Cambridge in the first half of the nineteenth century. It was chiefly through their activity that science lost some of the stigma that such as Priestley and Darwin had given it; the Cambridge brand of natural philosophy was quite amenable to such reactionaries as Coleridge and Wordsworth.<sup>26</sup>

Two members of the 'Cambridge Network' were particularly outstanding in this respect. John Herschel and William Whewell were 'two of the most important early Victorian commentators on science'. The two had close personal and professional links and, despite the apparent contrast between Herschel's 'empiricism' and Whewell's 'Kantianism', their thought had much in common. Their agreement on matters of natural philosophy 'drew its matter from Newtonian mechanics and astronomy, its form from the legacy of Newton's natural theology and rules of reasoning.'<sup>27</sup> Specifically, both Herschel and Whewell upheld the centrality of the concept of force and the need to search out the 'true cause' of phenomena.

In 1825, Whewell reflected on the 'view entertained by those who do not see the necessity for a perpetually sustaining and preserving providence in the world'. These preferred the notion that the material portion of the universe acted according to its own properties to produce a world. By ascribing natural phenomena to 'laws', they claimed to have explained the universe as 'an assemblage of matter merely, and have removed the necessity for considering an immaterial and intelligent agent one step farther from us, while we have brought the idea of the world as a machine much more distinctly into our view.'<sup>28</sup> It was this theological implication that led Whewell to reject this version of natural law.

Nor would he agree with Hume that laws were merely subjective constructs from phenomenal regularities. Laws were objective; however, they did not inhere in matter. Rather, they were the rules according to which 'an intelligent being by whom the material world is governed' acted.<sup>29</sup>

Whewell stressed this idea of the **government** of nature. 'Inert matter' did not 'observe' laws, as if voluntarily; God **caused** matter to act and work in specific ways. The deity was 'the agent of every single fact, and every single fact ..... [is] under the immediate guidance of his finger.' This conception of natural law, Whewell concluded, was the best response to those who would 'deify as it were the

maxims of ..... [God's] government' and who denied that the deity 'governs at all'.<sup>30</sup>

Whewell expanded on these themes in the volume that he contributed to the 'Bridgewater Treatises'. He interpreted natural law in an overtly anthropomorphic way; law in nature implied a law-giver and revealed 'something of the character of the power which has legislated for the material world.'<sup>31</sup> Whewell looked to Newton for an account of the means whereby these laws were executed. It was necessary to suppose 'some cause, independent of atoms themselves' which determined their properties.<sup>32</sup> Matter was dependent on some higher entity for its potency.

Ultimately, this power derived from God; 'laws' were the instruments of his government. Their operation testified to the presence 'at all times and in all places where the effects of the law occur' of the being who had devised and who sustained them: 'the knowledge and agency of the Divine Being pervade every portion of the universe'. God's presence was 'the necessary condition of any course of events, his universal agency the only origin of any efficient force.' In Whewell's, as in Newton's cosmology, the will of God was supreme.<sup>33</sup>

Whewell later tried to give a formal statement of this ontology. In his **Philosophy of the Inductive Sciences** (1842) he enshrined the categories of the Newtonian world-view in an **a priorist** theory of knowledge. While his earlier writings had been directed towards the 'materialist' position, exemplified by the anonymous **Vestiges of Creation**, in which natural law was raised as a rival to divine power; the **Philosophy** was a repudiation of the empiricist critique of Newtonianism. Whewell predicated his argument on the claim that experience could not be the sole source of knowledge: knowledge was universal and necessary whereas experience only gave access to the particular and the contingent. The qualities of necessity and universality had to be referred to the 'Fundamental Ideas' with which the mind was from the first equipped and which 'entirely shape and circumscribe our knowledge; they regulate the active operations of our minds, without which our passive sensations do not become known.'<sup>34</sup>

Thus the idea of 'cause' was only partly derived from the constant conjunction of certain phenomena; it also relied upon an intuition prior to experience. This fundamental idea supplied the notion of cause as 'some quality, power or efficacy, by which a state of things produces a succeeding state'; that is, it was the source of the belief in **efficient** causation as opposed to mere sequence. For instance, the motion of bodies had to be referred to a cause called 'force', such as the 'force of gravity' which caused

bodies to fall to the ground. Such causes, Whewell insisted, were real powers in nature.<sup>35</sup>

Moreover, causes were not to be conceived as either homogeneous or as autonomous. Instead, Whewell again echoed Newton in arguing that it was possible to go beyond any proximate cause to an ulterior one and that the former was subordinate to the latter. All the powers of nature could be so arranged into a hierarchy. This procedure led eventually to the

assumption of a First Cause, as an Axiom to which our Idea of Causation in time necessarily leads. And as we were thus guided to a First Cause in order of Succession, the same kind of necessity directs us to a Supreme Cause in the order of causation.<sup>36</sup>

The epistemological and the cosmological routes therefore led to the same point. The structure of the mind, as well as the structure of matter, testified to the existence of substantive powers in nature which were irreducible to either the phenomenal or to the material. These forces had to be assigned a spiritual base: they were the tokens of the over-arching power of God in the cosmos.

Herschel also subscribed to a realist interpretation of cause and force and to a spiritualist ontology. He was aware of the prejudice against natural philosophy that had arisen from its association with materialism; some scientists, he admitted, had cast doubts upon the separate existence of spirit. But he maintained that the 'natural effect' of science was to place 'the attributes of a Deity on such grounds as to render doubt absurd and atheism ridiculous.'<sup>37</sup>

Through science man could be led to 'the conception of a Power and an Intelligence superior to his own, and adequate to the production of all that he sees in nature.'<sup>38</sup> In particular, the search for the *verae causae* of phenomena tended in this direction. Herschel sided with Newton in maintaining that there were 'causes recognized as having a real existence in nature, and not merely hypotheses or figments of the mind.' Motion, for instance, had to be referred to the cause called 'force'.<sup>39</sup>

The nature of this force could be specified partly by exclusion and partly by analogy. Herschel remarked that matter had apparently two contradictory attributes: activity and inertia. However, the contradiction disappeared when it was recognized that the activity which matter displayed was the effect of the properties of another entity — force. This did not inhere in matter constantly as did inertia, but was ever on the decay; it was necessary, therefore, to conceive force as something which

was perpetually added to matter, without which all activity would cease.<sup>40</sup>

Since force was not a property of matter, it must have another provenance. In identifying this, Herschel relied upon the arguments of Common Sense philosophy. He alleged that human causation was the only case in which we have direct knowledge of the nature of physical power; there it was clear that 'something is going on within us, of which the mind is the agent, and the will the determining cause'.<sup>41</sup> By analogy, mind, acting through will, was the motor of the entire universe: matter was but a 'subordinate agent'.<sup>42</sup>

When, in 1833, Herschel discussed planetary motion in his *Treatise on Astronomy*, he invoked the same conceptions. He discussed the powers which caused heavenly bodies to 'deviate continually from the directions they would naturally seek to follow, in pursuance of the first law of motion, and bend their courses into curves concave to their centres.' Like Newton, he attributed to matter itself only the capacity to remain at rest or, once put in motion, to follow a straight line. All deviations from this, of which the elliptical orbits of the planets were the foremost example, had to be explained by the action of a force. Herschel rejected the view of certain 'metaphysical writers', like Thomas Brown, who had tried 'to reason away the connection between cause and effect, and fritter it down into the unsatisfactory relation of habitual sequence'.<sup>43</sup> Force as the real cause of motion was the irreducible postulate of mechanics.

Again Herschel argued that it was 'our own consciousness of effort' when the will moved the body or some other matter which provided an indispensable insight into the nature of power in the universe. But, on this occasion, he went further. In a passage that was to become famous and was frequently quoted in the natural philosophy of the next thirty years, he insisted that this experience of personal effort

compels us to believe that whenever we see material objects put in motion from a state of rest, or deflected from rectilinear paths, and changed in their velocities if already in motion, it is the consequence of such an EFFORT somehow exerted, though not accompanied with our consciousness.<sup>44</sup>

Just as human agency, mediated by the body, supposed the existence of mind and will; so natural agency, mediated by force, implied the existence of a superhuman intelligence executing its volitions



in the material world.

Herschel also insisted upon the need for the **continual** agency of this mind. He noted the tendency to decay and ultimate dissolution inherent in the planetary system — another Newtonian theme. The conservation of order was ‘brought about by the continued action of causes’ in which ‘we trace the Master-workman with whom the darkness is even as light’. In other words, God never slept: his constant supervision was essential to the perpetuation of the cosmos.<sup>45</sup>

As President of the British Association for the Advancement of Science in 1842, Herschel tried to impress his views of causation on his fellow scientists. It was, he maintained, ‘high time that philosophers, both physical and others, should come to some nearer agreement than appears to prevail as to the meaning they intend to convey in speaking of causes and causation.’<sup>46</sup> He acknowledged that some natural philosophers were content to look for ‘laws’ in the sense of regularities in phenomena, but he denied the adequacy of this notion. Such laws merely expressed a rule of action; they did not account for the action itself, and some notion of agency was central to scientific explanation. Either directly, or through delegation, Herschel argued, ‘whatever takes place is not merely **willed**, but **done**’.<sup>47</sup>

Later, Herschel specified what such an ‘agency’ theory of causation must imply:

The **presence of MIND** is what solves the whole difficulty; so far, at least, as it brings it within the sphere of our own consciousness, and into conformity with our own experience of **what action is**.

Only the analogy of spiritual volition could make physical processes intelligible.<sup>48</sup>

Herschel’s and Whewell’s versions of Newtonianism were not the only ones available in early nineteenth century Britain. They were, however, the sources upon which derivative writers tended to rely. Other philosophers went farther still in insisting on the primacy of force in nature: the school of dynamical philosophy pioneered by Humphrey Davy was the most obvious example of this trend.

Davy collaborated with Coleridge and absorbed his conception of natural theology. Davy held his philosophy to be Newtonian; however, while Herschel and Whewell developed Newton’s doctrines on the dependence of matter on force for its actions, Davy took up Newton’s speculation that matter might be as dependent on force for its very **being**. It was a small step from the suggestion that the

quantity of matter in the universe was so small that it could be contained by a nutshell to the claim that matter was entirely reducible to force: that extension and mass, as well as mobility, were attributable to the dynamic element in nature.<sup>49</sup>

Moreover, this force was, in Davy's words, 'an energy of mutation impressed by the will of the Deity'. Such dynamism was, therefore, a means of enhancing further the dependence of the natural upon the spiritual. For Davy, still more completely than for Herschel or Whewell, 'the power of God was the ultimate cause of the powers of matter', and the exercise of this power was not confined to an original act of creation but was a 'constant presence' whereby the universe was maintained.<sup>50</sup>

Thomas Exley gave a detailed exposition of this conception of matter in 1829. Matter, he held, was perceptible to man only because of its powers; indeed, these powers 'appear to constitute the very essence of matter.' He therefore found no use for solid atoms: the concept of matter was reducible to mathematical points from which forces acted.<sup>51</sup> One of Exley's concerns was to refute the argument that atoms were eternal and that matter could, in consequence, be conceived as independent of any 'creating power'. On a dynamical conception of matter, he asserted, there was

no difficulty in conceiving, that a Being, infinite in power and wisdom and every way perfect could enclose or fence up a small or large space by invincible power, so that none but himself could break the barrier, and towards this, or from it, he could thus produce one atom, so equally could he bring into existence an endless variety and multitude, and this presents us with the most common and familiar idea of matter.<sup>52</sup>

Although this kind of dynamism was usually associated with Boscovich, Newton had also approximated to a very similar position in which God, acting through space, endowed certain portions of it with the perceptual properties of matter.<sup>53</sup>

Moreover, dynamical theories of this type did not diverge from the standard Newtonian view of how nature was regulated. On the contrary, it enhanced the scope of divine action, because

matter exists continually by the power of its great Author ..... the same power which produced its existence is unremittingly requisite for the continuance of its being, so that, he not only made all things, but upholdeth all things, by the word of his power.<sup>54</sup>

Dynamism therefore completed the tendency inherent in Newtonian natural philosophy towards

the total subordination of the material to the spiritual; on this view, matter was left no remnant of autonomous being. However, as a resource in the natural theology of the time, dynamism was less used than the conventional matter-spirit dualism. It enjoyed a revival late in the nineteenth century when electrical theories of the atom afforded another opportunity for the dematerialisation of matter.

The most used element of Newtonianism in the early nineteenth century was the notion that power, whether conceived cosmologically as force or epistemologically as cause, was dependent upon the exercise of divine will. In the words of one theologian, it was only as 'the expression of such a Will or Power that the physical order of the universe is recognised as caused'.<sup>55</sup> The study of the natural world led, therefore, to the appreciation of a higher realm of being.

In general, this procedure could be used to validate the Christian dogma of a transcendent God. However, it also had a more particular use which deserves special notice. God was, in this style of natural theology, conceived as the **ruler** of the universe: the **control** that he exercised over nature was emphasised, and natural laws were approximated to the commands of human government. Moreover, the idea of divine rule was extended into the moral realm. 'Moral laws' were another product of the divine will, another feature of his sovereignty over creation and of his providence for man. In Tulloch's words, there was 'an eternal order in the government of the world', an 'immutable moral order', as well as a natural order.<sup>56</sup>

Certain 'social evils', like poverty, might lead to doubts as to the divine government of history; just as natural evils led to similar doubts about God's providence in nature. But a little casuistry soon removed such difficulties; it revealed that social evils were 'merely the negative side of that general condition upon which the whole advance, and even the very existence, of civilisation depends'. For example, even the 'extreme disproportion' of wealth in contemporary Britain could be referred to the need to reward the most industrious more generously than the rest. A 'certain inequality of social condition' was 'the appointment of Divine Wisdom'.<sup>57</sup>

Theodicy thus turned into a justification of social arrangements. The order of the natural and of the physical worlds were both necessary and good because of their origin in the divine will; their necessity derived from God's invincible power, their goodness from his beneficence. However, there was

a difference between the two: while matter **must** do as God commanded, man **ought** to obey. He had the freedom to flaunt the divine will; but, given God's power, he did so at his own risk. The way in which the implications of God as the sovereign of nature were employed in conservative political thought in Victorian Britain is considered below.

The ease with which discourse passed from the physical to the moral reveals much of the status of natural knowledge in the culture of early nineteenth century Britain. There was no strong boundaries between 'science' and theology and social thought; on the contrary, all existed in a 'common context' where it was assumed that there must be a considerable overlap between all of these fields.<sup>58</sup> There was, moreover, a clear hierarchy between the various forms of knowledge: theological truth took precedence over natural. Thus, for Michael Faraday, Davy's disciple,

the thorough-going divorce of science made absolument no sense .....

[Faraday] realized that to distinguish science from religion was not to sever them, but only to indicate the latter's absolute and logical primacy, while limiting the former's sphere.<sup>59</sup>

This was, however, only one version of what the cultural relations of science should be; a version which involved a certain social affiliation for natural knowledge. On this view, science formed one part of the 'Liberal Culture' propagated by the ancient universities and patronised chiefly by the clergy and the aristocracy. Philosophers like Herschel and Whewell were part of this system and contributed to one of its most characteristic products, natural theology. This form of cultural production at once presented an example of 'liberal' learning and expressed the interests of the established elites in Britain in 'moral order'; that is, in the perpetuation of the current distribution of power and wealth.

In contrast, during the nineteenth century an alternative notion of the proper cultural status of natural knowledge developed. This too had social connotations: it was the property of the swelling bourgeois and professional interest in Britain. While Oxford and Cambridge were the centres of the old view, the new industrial towns were the spawning-grounds of new conceptions of the cultural role of natural knowledge.<sup>60</sup> While the former context stressed the affinities of science with a wider field of knowledge, the latter increasingly presented it as an autonomous enterprise. In place of the

'moral' character of science in the aristocratic and clerical version, the middle-class was more concerned with its utilitarian applications.

The conflict between various notions of the cultural status of natural knowledge in the nineteenth century was in consequence

essentially a class struggle in which the ideals of the aristocracy to which the majority of men of science in the early nineteenth century subscribed, were gradually displaced by those of the industrial and commercial middle classes from whose ranks scientists were increasingly drawn.<sup>61</sup>

From the 1850s onwards, this struggle assumed a more particular character. The British middle class had become sufficiently differentiated internally for a body of professionals, or would be professionals, to exist outside the long-established fields of the law and the Church. These sought to appropriate various portions of culture as their own special field. In the case of science, there was an attempt to defeat decisively the aristocratic and clerical version of the cultural status of science, as epitomised in the natural theology of the time, and to assert that nature was the exclusive property of a scientific profession.

Under the old regime, men of science had been content to see themselves as 'individuals with a particular intellectual interest, in natural knowledge, which formed one not very significant segment of generally accepted cultural activity.'<sup>62</sup> Increasingly, however, they sought both a new social status for themselves and a new concept of their professional knowledge. While a former generation of natural philosopher had sought a living and a social identity in the Church, like Whewell, or by assimilation to the aristocracy, like Davy; the new men were not prepared to pursue their science as a subsidiary to another occupation or under the patronage of gentlemen. They looked for full professional standing for their discipline and assured means to practice it.

At the same time, while the former generation were ready to accommodate their knowledge to the Christian world-view, their successors rebelled against it. Natural theology was identified as an aspect of the subordinate unprofessional character of British science. On the other hand, a naturalistic outlook was presented as the concomitant of professionalism. This claim and the response to it from other interest groups was central to the debates about cause and force in the mid-Victorian period.

### iii. Theological Responses to the Naturalist Programme

There were various indications after 1850 of the move to redefine the cultural relations and social status of science. Influential bodies were effectively taken over by the advocates of professionalism and used for the dissemination of their views. The Professorship of the Royal Institution, previously held by Davy and Faraday, passed in 1853 to John Tyndall. Under his guidance, the Royal Institution provided a platform for spokesmen of naturalism like W.K. Clifford and T.H. Huxley, as well as Tyndall himself. Each in his own way put forward a conception of naturalism as a comprehensive and sufficient account of the world, and of science as an autonomous intellectual pursuit.

Similarly, the annual meetings of the British Association for the Advancement of Science provided an occasion for the announcement of the novel conception of natural knowledge. Thomas Hirst noted at the BAAS at Bath in 1864 'the applause with which every protest against fettering science by religious dogmas was received'. The editor of the **Bath Chronicle** observed the same facts with less enthusiasm. He wrote that a 'dangerous clique' was 'assuming a prominent place in the Association'; they endeavoured to 'use this grand Scientific League in furtherance of heretical teachings, as a prop to the Scepticism which has of late years met with disciples even in the ranks of duly authorised Christian Ministers.'<sup>63</sup>

This last reference was to the authors of **Essays and Reviews** and to other 'liberal' theologians who had upset orthodox religious opinions in recent years by their 'rationalist' attitudes. As argued in Chapter One, there were affinities between this group and the scientific naturalists, not only in the means they employed, but also in their goals. Both were professionalising movements seeking to devise a specialist disciplinary language untainted by 'extraneous' concerns.

At the 1868 BAAS at Norwich, the strategy of the 'dangerous clique' advanced a stage further. Joseph Hooker, in his Presidential Address, called for the detachment of scientific from theological discourse: he argued, for example, that scientific writers, should not, as some had done, declare against the Darwinian hypotheses on 'metaphysical' grounds which were 'strongly imbued with theological prejudice and even odium, and as such are beyond the pale of scientific criticism.'<sup>64</sup>

Hooker held that, although individual clergymen had contributed to science in the past, the roles of cleric and scientist were better kept apart. Each had his own sphere. It was for the scientist to enquire 'into the physical, the religious teacher into the spiritual history and condition of mankind'. Each should regard

the other's field 'from afar'. Conflict could be avoided only if the theologian recognised that 'the laws of matter are not within the religious teacher's province'. In particular, Hooker insisted, that both science and theology must eschew 'that most dangerous of all two-edged weapons, Natural Theology'.<sup>65</sup>

Religious writers had for some years noted this attempt to exclude theological principles from the realm of matter by certain scientists. An article in the *Westminster Review* of 1855 by Francis Newman observed a 'tendency towards Materialism in many minds of more purely scientific nurture'. Newman was, to an extent, sympathetic to this attitude; he recognised it as a reaction to the efforts of the defenders of the 'spiritual or Theistic theory' to 'impose on their [scientists] research conditions and restrictions which they justly and truthfully disowned'.<sup>66</sup>

The general theological response was less understanding. The proponents of scientific naturalism were accused of creating a false boundary between the spiritual and the physical and of presuming upon the prerogative of the theologian in stating the metaphysical framework within which science should proceed. Although overtly a limited operation, many theologians saw the naturalist strategy as a potential threat to the very existence of the spiritual realm which was their reason of being. Thus, Alfred Barry declared in 1869:

in England at the present day Physical Philosophy is <sup>c</sup>enroaching on a domain which is not its own  
<sub>X</sub>  
 ..... it is therefore becoming what we call 'Materialism', ie., ..... not a study of that which is Material,  
 but the desire to make all things Material – to ignore any distinction of kind between mind and  
 matter.

Such a materialism struck 'at the root and possibility of Theology'.<sup>67</sup>

James Martineau also saw in the naturalists' declaration of autonomy for science a threat to the bases of theology. In particular, he argued that if science were successfully confined to naturalistic accounts of phenomena, with no reference to a higher spiritual realm, then theology would be denied access to reality as a support for its doctrines; in effect it would cease to be knowledge and become mere belief. Martineau noted the treaty reached between naturalists and 'liberal divines' whereby a rigid demarcation between science and theology had been agreed; however, he rejected it. Not only did this arrangement deprive theology of a crucial resource, the natural world, it was also 'utterly destructive of the equipoise of authority between the two spheres, to characterize the one [science] as "knowledge", which involves objective certainty, the other [theology] as "faith", which goes no further than subjective assurance'.<sup>68</sup> To admit, as the liberal divines had

done, the claims of naturalism was tantamount to conceding the cultural inferiority of theology.

Martineau returned to this point when he commented upon Hooker's Norwich Address. Hooker, he alleged, had sanctioned the dictum that 'religion deals only with the inscrutable, and can make no terms with science, except by unconditional surrender of the whole field of intelligence, and withdrawal into the dark'. By declaring that the 'physical' could be understood with <sup>out</sup> reference to the 'metaphysical', Hooker had 'cut off all passage from nature to God'.<sup>69</sup>

Martineau, and others determined to defend the possibility of a natural theology, made the question of the nature of force and causation their chosen battleground. While, as Alfred Barry wrote, the notion that force was endogenous to nature 'excludes the idea of a superior will in the creation and government of the world'<sup>70</sup>; properly conceived, 'force' could provide a middle term between the scientific and the theological understanding. The scrutiny of natural forces, Martineau held, led to the recognition of their unity and to the realisation that volition was the type of all of them. From this Martineau, like Herschel before him, proceeded to the 'resolution of all external causation into Divine Will'.<sup>71</sup>

This conception of causation became one of the major bastions which the defenders of natural theology held against scientific naturalism in the mid-Victorian period. In effect, Martineau reverted to the Newtonian distinction between matter and force as a means of reintroducing spirit into nature. Given these assumptions, he argued, 'Natural Theology ..... is a perfectly legitimate exercise'; its task was to extract the moral implications of the working of the divine will in the forces of nature.<sup>72</sup> The natural theologian added to scientists' account of the world 'the embracing idea of intellectual purpose', and interfused 'an omnipresent Thought with the working powers which they have found'.<sup>73</sup>

Scientific naturalism developed two main responses to such arguments: one was materialistic, the other positivistic. Both attempted to deny 'force' and 'cause' any autonomous status in scientific discourse; but while the one tried to reduce force to a property of matter, the other proposed a still more radical solution. It questioned the epistemological assumptions made by Martineau and his kind, and tried to create a model of scientific knowledge which was unamenable to such extrapolations.

These two moves were, in fact, closely intermingled. However, for the sake of exposition, it is convenient to treat them separately.



#### iv. The Naturalism of Substance

In 1865 Herschel wrote that there were two main challenges to the view of causality which he espoused. One was the theory that 'all we can ever attain to is the observation of and registry of constant laws of phenomenal sequence'; the other was the claim that all force could be reduced ultimately to the motions of matter. According to this doctrine, heat, light, electricity, as well as kinesis, were resolvable to 'the mere knocking about of an inconceivable number of inconceivably minute billiard balls'. These, 'once set in motion and abandoned to their mutual encounter and impact, work out the totality of natural phaenomena.'<sup>74</sup>

In a sense, this was a return to the 'Lucretian' atomism of the seventeenth century, which held that atomic motion was sufficient to constitute the world. However, Herschel noted another element associated with this model that was of more recent provenance. The obvious objection to the sufficiency of atomic motion as the basis of the universe was that the *vis viva*, or active power, of these particles must eventually be exhausted. At that time, all activity would cease. The materialists had, however, an answer to this: they claimed that *vis viva* was never destroyed but only converted into another form of energy.<sup>75</sup> This doctrine of the conservation of force, or of energy, was a relative novelty in mid-Victorian Britain.

Martineau too was aware of this brand of materialism. He regarded it as a crude amalgamation of the atomic and the conservation theories designed both to remedy some of the defects of the former and the obviate truly dynamic ideas. This conception reduced causation to an account of the successive states of material systems and excluded any true notion of agency.<sup>76</sup>

The connection between atomism and conservation theory in Britain was of long standing. The concepts of the interconvertibility of heat, motion and other forces devised by such Continental workers as Clausius and Helmholtz, were from the first related to a native tradition of corpuscular philosophy in Britain. For instance, it was pointed out that Carnot's experiments had been 'anticipated' by Davy's researches on heat. These had led Davy to the conclusion that 'heat and cold may be defined [as] a peculiar motion, probably a vibration, of the corpuscles of bodies, tending to separate them'.<sup>77</sup>

Similarly, W. R. Grove, who in 1842 gave one of the first expositions of conservation theory in Britain, chose to identify the doctrine with that of the 'correlation of force'. Forces could be converted

into one another because they were all species of motion, whether gross or atomic. On this view, it seemed that forces were not something apart from matter; nor were they introduced into nature from outside. On the contrary, it was 'an irresistible inference from observed phenomena that a force cannot originate otherwise than by devolution from some pre-existing force or forces.'<sup>78</sup>

Tyndall in 1863 worked out these ideas in greater detail. Tyndall had studied in Germany in the 1850s where he had come 'face to face with the great generalisation of the Conservation of Energy'.<sup>79</sup> Nonetheless, his exposition remained characteristically British, in that he linked the conservation of energy to the supposed kinetic nature of all forces and to an atomic theory of matter. Tyndall also showed from the first a determination to derive broad cosmological principles from these doctrines. In his 1862 Royal Institution lectures on **Heat Considered as a Mode of Motion**, he held that the 'new philosophy' which he described could not be confined to heat alone: through an understanding of this agent insight was gained into 'the general energies of the universe'.<sup>80</sup>

The fundamental principle of the new philosophy was that heat was not matter, 'but an accident or condition of matter; namely, a **motion of its ultimate particles**.' This motion could be converted into other kinds of movements which underlay different forces; thereby natural power was conserved, apparent waste being merely conversion.<sup>81</sup>

Tyndall held that Helmholtz's comparison between 'potential' and 'actual' (or kinetic) energy was the most general expression of this fact. At this point the joins in Tyndall's system began to show. Although Helmholtz's notion of the conservation of energy **could** be coupled with kinetic and atomic foundation both in principle and historically the two theories were quite separate. Indeed, the tendency of Helmholtz's own thought, as shall be seen, was toward a rejection of any atomic mechanism as an 'explanation' of the conservation of energy in nature.

The external character of the association between the two elements of Tyndall's theory was clearly revealed in a popular paper on 'The Constitution of Nature' published in 1865. The piece fell into two distinct parts. The first contained an account of the structure of the cosmos which had clear affinities with older materialist systems. Nature was a plenum: 'space' was in fact the locus of an ether which displayed the mechanical properties of a frictionless jelly. This ether mediated impulses between bodies; it was also the means whereby light and heat were transmitted. Matter was held to be eternal:

its origin could not be traced to 'the fiat of Omnipotence'. Further, matter was in a 'state of incessant tremors'; its atoms were in constant motion, and the different forms of force could be referred to the various frequencies of these vibrations.<sup>82</sup>

Tyndall presented the theory of conservation as a complement to such a materialist cosmology. Joule's discovery of a mechanical equivalent of heat was a measure of the perfect transference of motion between bodies and the parts of bodies; it showed why nature's activity was never exhausted although constantly changing. However, Tyndall's account of energy, and of how it was 'conserved', was effectively independent of the cosmology of the first half of the paper. Such concepts as 'kinetic energy' were defined in terms of the **relations** between objects and of such inherent properties as mass.<sup>83</sup> These definitions needed neither atomism nor an ether; still less was a kinetic theory of force necessary to their comprehension and application.

This was even more obvious in Tyndall's discussion of the connection between potential and kinetic energy, the key to the theory of conservation. Broadly put, he declared, 'the principle of the conservation of force asserts, that the quantity of force in the universe is as unalterable as the quantity of matter; that it is alike impossible to create force and to annihilate it.'<sup>84</sup> But, if this were the case, how was it that certain forces, such as gravity, seemed to vary within very wide limits? Did this not imply that force was something which was lost in action and had to be replenished from some other source?

Tyndall's reply to this objection was taken almost verbatim from Helmholtz. Tyndall posited two points, D and F, the latter of which was fixed while the former was mobile. These were located at a certain distance apart and were assumed to have mass. The attraction between them therefore varied inversely with the square of the distance. At every point between F and D it was possible to draw a perpendicular whose length, in accordance with mechanical convention, would be proportional to the attraction, or 'tension' at this point.

When D was at a virtually infinite distance from F, the attraction between them was near to nil. At that moment, the **vis viva** of D would also be minimal; but, at the same time, the sum of the tensions between F and D or the **potential energy** of the system would be at its maximum. As the distance between D and F decreased, so would these tensions be progressively consumed and the potential

reduced. D would realise the potential for motion inherent in its original relation to F and would thereby acquire **kinetic energy**. Thus, the 'nearer D approaches to F, the smaller is the sum of the tensions remaining, but the greater is the **vis viva**; the farther D is from F, the greater is the sum of the unconsumed tensions, and the less is the living force.'<sup>85</sup>

This formulation, Tyndall contended, solved the problem of the apparent appearance and disappearance of force. The theory of the conservation of energy required neither that **vis viva** nor that the tensions in nature were unvarying: it did show that the **sum** of these two quantities was constant. The universe possessed 'two kinds of property which are mutually convertible. The diminution of either carries with it the enhancement of the other, the total value of the property remains unchanged.'<sup>86</sup> The dynamic of nature consisted, therefore, in an oscillation between potential and kinetic energy; although the proportion of each in any system might vary with time, there was an overall equilibrium. The cosmos was a self-contained, self-perpetuating mechanism.<sup>87</sup>

There was no reason, Tyndall argued, to invoke some 'occult quality' to account for accelerated motion. The concept of force was reducible to a property of material systems; this eliminated the notion of force as an agent or entity independent of matter. However, it also excluded the need to refer to any putative atomic base of mechanical phenomena: 'matter', on this view was reducible to points in space with mass. In effect, the principle of conservation, so conceived, exemplified a model of physical discourse radically different from both the spiritualism of Herschel and the materialistic aspect of Tyndall's thought. It abstracted from the phenomena of dynamics their essential characteristics and, without trying to 'explain' them, offered a generalised **description** of what took place. On this view, it was ~~unnecessary~~ to specify whether energy was an autonomous entity or a property of matter; it was merely a symbol for the capacity for change displayed by bodies under certain conditions. Tyndall did no more than hint at such a philosophy; its systematic development as a part of the strategy of naturalism was left to others.

Tyndall's own polemical use of the principle of conservation had both a particular and a general aspect. He employed the theory in the controversies of the 1860s over the physical efficacy of prayer. At stake were the rival claims of scientists and clergy to possess the means to remedy natural ills. The permanence of force, as deduced from the principle of conservation, was used to invalidate the belief that

God could intervene in any part of the natural world to influence events by his direct volitions, whether or not he was implored to do so by his earthly representatives. Instead, society should turn to science to relieve disease and other natural calamities, and supply appropriate rewards to men of science.<sup>88</sup>

This was a special instance of the immanentist consequences that Tyndall drew from modern natural philosophy. He argued that the motive and regulative powers of the world should not be conceived as outside the material universe, but as inherent in and constitutive of it. Carlyle's notion of nature as a tree, whose principle of growth lay within, was preferable concept of nature as a machine governed by an external agent.<sup>89</sup>

Energy could be considered as this inner power from which all activity proceeded. It was, Tyndall held, the 'Proteus' which underlay all change. The principle of conservation revealed immutability in the midst of change, [it] recognises incessant transference and conversion, but neither final gain nor loss. This law generalises the aphorism of Solomon, that there is nothing new under the sun, by teaching us to detect everywhere, under the infinite variety of appearances, the same primeval force. To Nature nothing can be added; from Nature nothing can be taken away ..... The law of conservation rigidly excludes both creation and annihilation ..... the flux of power is eternally the same. It rolls in music through the ages, and all terrestrial energy, — the manifestations of life, as well as the display of phenomena, — are but the modifications of its rhythm.<sup>90</sup>

The distinction between creator and creation was thus annihilated. 'God', in as far as that term had any application, was but the name for the inherent impulse and tendency of matter. These ideas had obvious affinities with the German Romantics' pantheism, but they also had deeper roots. This philosophy bore a striking resemblance to the hylozoism of such Renaissance Hermeticists as Giordano Bruno. For Bruno, as for Tyndall, the universe was in constant flux, whose cause was an inner principle the manifestations of which might change but whose essence was eternal.<sup>91</sup>

Tyndall acknowledged the provenance of his world-view in the most notorious of his utterances: the Address he gave as President of the BAAS at Belfast in 1874. The Address was the culmination of the rhetoric of professionalisation outlined above; it was delivered at a time when the conflict between

scientists and clerics was particularly intense and the need for a declaration of the rights of science greatest.<sup>92</sup> Tyndall put the aspirations of science succinctly:

We claim, and we shall wrest from theology, the entire domain of cosmological theory.

All schemes and systems which thus infringe upon the domain of science must, in so far as they do this, submit to its control and relinquish all thought of controlling it.<sup>93</sup>

He made the opinion that matter was a sufficient basis of all phenomena a central resource in this effort to assert the autonomy of science from theology and its sovereignty over nature.

Tyndall presented the history of science as a conflict between 'true' natural philosophers, who attempted to 'connect natural phenomena with their physical principles', and those who introduced 'transcendent', anthropomorphic, notions into scientific explanation. The latter refused to make matter self-reliant but invented a 'mob of gods and demons' to control events.<sup>94</sup> Christianity had, in general, been inimical to scientific thought: it had referred natural events to 'moral' rather than to physical causes. Only in the sixteenth century had the pure strain of natural philosophy reasserted itself.

Among the thinkers of that century, Tyndall maintained, none had come nearer to 'our present line of thought' than Bruno, who had concluded that

Nature, in her productions, does not imitate the technic of man. Her process is one of unravelling and unfolding. The infinity of forms under which matter appears was not imposed on it by an external artificer; by its own intrinsic force and virtue it brings these forms forth. Matter is not the mere naked, empty capacity which philosophers have pictured her to be, but the universal mother, who brings forth all things as the fruit of her own womb.<sup>95</sup>

For holding such views, Bruno had been burned as an heretic; his was an extreme case of the repression of science by theology. But the basic conception which he had originated survived: namely, the notion that the 'principle of every change resides in matter', and that, while in artificial productions, 'the moving principle is different from the matter worked upon; ..... in nature the agent works within, being the most active and mobile part of the material itself'.<sup>96</sup>

This doctrine embodied a cultural demarcation. Just as matter was independent of external agency for its action, so the study of the material could proceed according to its own endogenous

principles. This was the essence of Tyndall's claim that the 'entire domain of cosmological theory' should be recognised as belonging to science alone and as subject to no extraneous controls. Specifically, a strict exclusion of theology from science was demanded.

The clerical reaction to the Belfast Address was shrill: 'Cardinals, Archbishops, Bishops, the lesser clerics and laymen joined in the hue and cry in pulpit and press'.<sup>97</sup> In part, this outrage arose from the audacity with which Tyndall had flaunted Christian doctrines in such a public forum. However, the response of hostile theologians showed that they had also grasped the broader issue at stake. As in their earlier responses to scientific naturalism, their criticism was voiced in the language of the violation of proper cultural boundaries and the erection of false ones. Thus Henry Wace declared that Tyndall had abused the office of popular exponent of science when he 'introduced his speculations into regions far beyond those which are properly the province of the Professor of Natural Sciences'; he had dared to venture into 'the superlunary realms of metaphysics' and to pronounce upon the ultimate suppositions of science.<sup>98</sup>

Robert Watts, Professor of Systematic Theology at the General Assembly's College in Belfast, stressed the obverse of Tyndall's strategy. While he eroded the restrictions upon the scientist, Tyndall tried to impose bounds upon the competence and authority of theologians. According to Tyndall and to other 'anti-theistic' writers,

The empire of matter is so peculiarly the heritage of science that the theologian has nothing whatever to do with it, and is to be treated as an intruder and trespasser when he crosses the boundary which, it is alleged, separates theology from science.<sup>99</sup>

Watts held that the antagonism between the scientific and the theological outlooks was reducible to 'the difference which subsists between men of science and theologians, in regard to the nature of the cause to which the phenomena of the universe are to be referred'. While the 'scientist' was content to refer causes to the properties of matter, the 'theologian' saw them as the instruments of a 'Personal Intelligence'.<sup>100</sup> This discrepancy was evident in the 'Belfast Address': Tyndall's object, 'like that of Epicurus', was to rid the world of divine power. While 'Epicurus aimed at the extirpation of the gods of Greece; Dr Tyndall aims at the extirpation of the Jehovah of the Bible'. From the theologian's viewpoint, the integrity of Christian dogma demanded the retention of a principle beyond the forces

and powers of nature: 'a devising mind, arranging the agency and determining the result'.<sup>101</sup>

To this end, the volitional theory of causation was again deployed; James Martineau was its foremost exponent in the later nineteenth century. Moreover, theologians of this persuasion received powerful support from certain scientists, who held that on **physical** grounds it was possible to show that the material world depended on a spiritual reality. Watts' facile distinction between 'scientific' and 'theological' attitudes obscured the fact that a commitment to natural theology survived among a section of the British scientific community to the end of the nineteenth century and beyond.

One stronghold of this group were the Scottish universities. During the nineteenth century these constituted centres of resistance to the tendency towards professionalism and specialisation at work in Britain. According to the Scottish pedagogy, truth was indivisible; education consisted in the drawing out of the connections between, for instance, mathematics and morality, rather than in inculcating the principles of any one narrowly conceived discipline.<sup>102</sup> Within this tradition, the close association between science and theology was unexceptionable; on the contrary, it was implicit both in the prevailing theory of knowledge and embodied in the structure of teaching.

The leading product of this conception of natural philosophy in the later nineteenth century was Balfour Stewart and P. G. Tait's **The Unseen Universe** (1875).<sup>103</sup> In the preface to the first edition of this much reprinted book, the authors declared themselves 'aghast at the materialist statements now-a-days freely made (often professedly in the name of science)', and at the neglect of 'the splendid example shown by intellectual giants like Newton and Faraday' in regard to the proper attitude of science to religion. In particular, Stewart and Tait took exception to 'the stupendous pair of assumptions that visble matter is **eternal**, and that **IT IS ALIVE**'.<sup>104</sup>

Their basic contention was that 'we must ..... assume the existence of a Deity who is the Creator and Upholder of all things'; natural laws should be seen as the means whereby God conditioned the universe. Nothing occurred 'which does not take place under the conditions imposed by the will of God'.<sup>105</sup>

Stewart and Tait tried to establish the notion of the 'Sovereign Power of God' by their own interpretation of the principle of the conservation of energy. While Tyndall had argued that this theory revealed matter to be sufficient to generate all the phenomena of nature, Stewart and Tait emphasised



the **dualism** inherent in the conceptions of physics. Matter formed only one part of the universe, and that the less important. The other moiety was constituted by energy which had 'as much claim to be regarded as an objective reality as matter itself.' Moreover, while matter was passive, energy was the active element in nature: through its transformations change occurred.<sup>106</sup>

Energy was lodged in and worked through the ether. However, this was not the simple medium which Tyndall had supposed whose workings were compatible with the properties of a frictionless jelly. By the 1870s, the ether had begun that series of transformations which, by the end of the nineteenth century, was to make it into something at once of supreme importance to physics and yet something which appeared anomalous to many basic physical principles.<sup>107</sup> Like energy, matter was dependent on the ether. The atom, according to Kelvin, was no more than a 'vortex ring'; that is, a convulsion in the ether.

In contrast, therefore, to Tyndall's one level universe, Stewart and Tait postulated three levels of reality between which determinate relations existed. Matter was inferior to energy, and both were dependent on the ether. Moreover, they were not prepared to stop there; the whole tendency of their argument was towards the claim that the study of the physical universe led 'the scientific mind ..... from the visible and tangible to the invisible and intangible'. The existence of vortex atoms in what must have been an originally homogeneous ether implied, they argued, an act of creation: 'that is to say, an act impressed upon the universe from without'. Thus the ether itself was a dependent entity; its inherent properties would never have given rise to matter or energy.<sup>108</sup>

The mind was led in this way to a yet higher realm of being — to belief in the existence of an invisible order of things intimately connected with the present, and capable of acting energetically upon it — for, in truth, the energy of the present system is to be looked upon as originally derived from the invisible universe, while the forces which give rise to the transmutations of energy probably take their origin in the same region.<sup>109</sup>

In this way the Newtonian hierarchy was preserved, though in a somewhat altered idiom. Matter and that which activated matter were held to be separate and to depend on a spiritual power. Stewart summarised the theological implications of this position in 1884, when he declared his allegiance to the hypothesis that everything was created and sustained 'by the immediate operation of Divine Power'.

On this view, 'the continued existence of the visible universe results from a virtual prolongation of that Divine operation which gave it birth ..... that is to say, from a continued exercise of the Divine will and of the Divine perception.'<sup>110</sup>

These were not the words of a philosopher or of a theologian but of an eminent physicist. Nor were Stewart's views an isolated eccentricity; many other of the foremost British physicists of the late nineteenth century showed a similar readiness to accept a spiritualist theory. The Cambridge physics school, which was imbued with Scottish principles and ideals of natural philosophy, was outstanding in this respect.<sup>111</sup>

This was a serious embarrassment to those who wished to claim that scientific progress was dependent upon the banishment of theological concerns from the study of nature. It was impossible to dismiss the natural theology of Stewart, Kelvin, or Lodge as the result of ignorance. However, the naturalists still had recourse to another tactic: they could shift the argument to epistemology and argue that these eminent scientists had failed to recognise the necessary limitations upon scientific discourse. Instead of asking whether, in fact, nature displayed evidence of spiritual guidance; they inquired whether, **in principle**, such a question was meaningful. The trend of naturalistic thought in the last decades of the nineteenth century was to assert that such exercises in natural theology as the **Unseen Universe** rested upon a misunderstanding of the potentials of the human mind and of the conditions of knowledge.

## v. The Naturalism of Method

[B]ehind the epistemological scholasticism of empirio-criticism one must not fail to see the struggle of parties in philosophy, a struggle which in the last analysis reflects the tendencies of and ideology of the antagonistic classes in modern society.<sup>112</sup>

(V. I. Lenin)

In the history of philosophy Hume's work is usually regarded as a turning-point even as the 'beginning of modern philosophy'. Certainly, his ghost haunted the nineteenth century: in both Britain and Germany philosophy tended to be, it not a series of footnotes on Hume, then a conscious attempt to define positions which either complemented or superseded his view of knowledge.

The 'Scottish and German responses to Hume' showed important affinities as well as differences.<sup>113</sup> The tendency of Common Sense philosophy to meet Hume's empiricism and scepticism with intuitionism and realism has already been discussed. It should be recalled, however, that even in Scotland Hume had his defenders, like Thomas Brown, who upheld the constant conjunction theory of causation against its critics. While Common Sense philosophy supplied a tool to those who wished to turn the concepts of force and cause to theological ends in the nineteenth century, so Hume's thought, as mediated by such interpreters, provided a resource to the proponents of naturalism.

In addition to these native traditions, Kant's critique of Hume supplied further materials. There was a basic ambiguity in the Kantian legacy which led to two views of his philosophy. On the one hand, Kant's stress upon the necessity of categories as the condition of intelligible experience was presented as a vindication of an anti-empiricist epistemology. The mind was something more than the sum of its experiences: human consciousness was a prior element in nature. Moreover, the universality of the contents of mind evinced the existence of a transcendental consciousness in whose intellect these categories originated. This conclusion was presented as a triumph for fideism on the Continent; in Britain, as has been seen, Whewell integrated this argument into the domestic concerns of natural theology.

But, just as there were several Newtons, so there were at least two Kants. Aside from the theist and transcendentalist, there was the Kant who accepted Hume's limitation of knowledge to the phenomenal; who recognised that the noumenal was, at most, an unverifiable hypothesis. This Kant

became the figurehead of a revulsion in later nineteenth century Germany against both absolute idealism and 'vulgar' materialism. The 'neo-Kantians' claimed to be pursuing the true intimations of their master's thought when they restricted the knowable to the contents of the sensorium and devised a philosophy of science which fitted these limits. Their example provided a supplement to homegrown versions of empiricism in Britain.

Discussion of the 'school of experience' in later nineteenth century Britain, whether hostile or friendly tended to start from the work of J.S. Mill. His preeminence depended only partly on the philosophical creed which he articulated.<sup>114</sup>

His major epistemological work was the *Logic* of 1843. This was a vast, dry effort with few obvious claims to general interest. However, the book attracted widespread and prolonged attention because it was recognised as an important contribution to a long-running controversy; a dispute, moreover, whose ramifications extended far beyond technical philosophy. Despite his disclaimers in the introduction of the *Logic*, Mill himself saw a polemical function for the work. In particular, he welcomed the response that the section on causation had evoked: he wrote in 1854 that 'it is good to see the subject stirred up and that book taking its place as the standard philosophical representative in England (unhappily the only one) of the anti-innate principle and anti-natural-theology doctrines.'<sup>115</sup>

Mill was thus concerned to refute the uses to which certain theories of causation had been put in the early nineteenth century. Specifically, he assailed the notion of cause which was most in vogue with 'the schools of metaphysics'; namely, that which supposed that something more than sequence was connoted by 'cause', and which asserted the 'necessity of ascending higher, into the essences and inherent constitution of things to find the true cause, the cause which is not only followed by, but actually produces the effect.'<sup>116</sup>

Mill's counter to such attempts to suppose a supra-empirical 'efficient' cause was, in essence, to restate Hume's criticism of the idea of necessary connection and to endorse his view of causation—when suitably 'improved'. Mill insisted that invariable succession between phenomena was the real basis of the idea of cause, irrespective of 'every other question of the nature of "Things in themselves"'.<sup>117</sup> Mill claimed to have improved upon Hume in his opinion that mere antecedence was not an adequate base for causality: to be a 'cause' a phenomenon had to be the invariable and **unconditional** antecedent

of another. By this qualification, Mill held that he had answered the most telling of Reid's criticisms of Hume.<sup>118</sup>

Mill was especially concerned to scotch the theory that volition was the type of causation. He saw this view as the main rival of his own; it had 'been revived during the last few years in many quarters, and at present gives more signs of life than any other theory of causation at variance with that set forth in the preceding pages'. Mill recognised the 'Newtonian' tendency of this voluntarism; that is, the implication drawn from it that

All things which do not proceed from a human ..... will, proceed ..... directly from divine will.

The earth is not moved by the combination of a centripetal and a projectile force; this is but a mode of speaking, which serves to facilitate our conceptions. It is moved by the direct volition of an Omnipotent Being.<sup>119</sup>

However, as Hume had shown, there was no reason to regard the causal status of the will in a different way from that of any other phenomenon. All were capable of being causes if they stood as the invariable and unconditional antecedents of an event. To go further and to assert that the will had a **power** to produce certain events was to go beyond the physical evidence.<sup>120</sup>

This was the model of later empiricist assaults on the elevation of will to a special, theologically significant, position in nature. There were, however, different emphases among those who pursued this strategy. Alexander Bain, for example, who was in most things Mill's seconder, shifted the argument from logic to psychology. However, given the psychological bases of empiricist logic, this was not a drastic move. In 1859 Bain noted that 'a singular importance' had been attached to the feeling of effort by certain philosophers: they had asserted that 'our own volition is to be taken as the type of moving power, even in the inanimate world'. Specifically, Bain addressed the passage in Herschel's **Astronomy**, quoted above, where consciousness and will were supposed to be the cause of all accelerated motion.<sup>121</sup>

Like Mill, Bain denied that any special importance could be attached to the fact that, in certain cases, feelings of volition and effort preceded effects. But Bain looked to physiology to sustain his theory of the real relations of consciousness to the rest of nature. He pointed out that the voluntarist argument was difficult to sustain even for the supposed government of the body by the will.

Consciousness was only one precondition of action; moreover, it was not an invariable precondition: in certain cases, action occurred without any conscious volition. On the other hand, 'organic energy' and the material mechanism of the body were invariable conditions of movement; while these were fundamental facts of bodily motion, 'consciousness is the occasional and accessory fact related to them.'<sup>122</sup>

If an analogy were to be drawn between the body and the rest of nature, therefore, there was no reason to fasten upon the incidental facts of consciousness as the most relevant. On the contrary, the material conditions had a much greater claim to attention. It was true both of the body and of the cosmos generally that 'the physical energy is the indispensable condition [of movement], and the consciousness the casual.'<sup>123</sup>

In some respects G. H. Lewes's 'positive philosophy' was but a continuation of Mill and Bain's line of argument. Lewes elaborated the idea that the cause of an event could not be conceived as something autonomous but had to be regarded as an aspect of phenomenal sequences themselves. Reid and Herschel had made the error of supposing that for every event there must be an 'agent behind the scene', whereas there was only the scene.<sup>124</sup> The notion of will as cause was but a special case of this mistake; Lewes rejected it because it involved the hypothesis of 'an Efficiens which is not Materia — a Will apart from its known conditions — and supposes that the material changes we observe are the products of this immaterial Efficiens.'<sup>125</sup>

However, Lewes held that Mill in his theory of causation had not been sufficiently thoroughgoing an empiricist. Mill had held that although causal sequences were not necessary, they were objective. Lewes, in contrast, wished to stress the **conventional** nature of causal laws. The phenomenal world was, he argued almost infinitely complex and could be sub-divided in many different ways. It was wrong therefore to suppose that cause and effect were given in phenomena themselves; on the contrary, 'cause' and 'effect' could be 'the same phenomena differently viewed ..... ; the action and the act are but diverse aspects of the event'.<sup>126</sup>

The object of science was to cut up this seamless web in ways which would maximise the intelligibility of events: 'laws' were imposed by the mind on the materials of experience. The constitution of 'nature' by this means had two stages: the first involved the classification of sensibilia into a number of

categories. The second consisted in the invention of further concepts which related directly to no object of experience but which were needed to make some phenomenon comprehensible. These concepts had, therefore, an overtly instrumental role and nominal status.<sup>127</sup>

Thus, at one level, 'matter' was nothing but the sum of the sensations which had been collected under that name. However, philosophers further hypothesised 'atoms' and other entities, which had never been perceived, but which helped to elucidate sensation. Lewes held that such concepts were unobjectionable so long as philosophers remembered that they were merely artifices 'by which we introduce congruity into our symbols, and bring a variety of phenomena under one set of quantitative dynamic symbols.'<sup>128</sup>

Besides matter, Lewes held that there were three other fundamental concepts extrapolated from experience: force, position, and motion. He considered force to be a product of what, earlier in the nineteenth century, had been called 'muscle sense'; that is, the experience of muscular pressure gave rise to dynamic conceptions. Without this kind of experience, 'we might have a conception of the geometric universe, but we would have none of the dynamic universe.'<sup>129</sup> Lewes thus conceded the Common Sense claim that we have a **real** experience of force; however, he altered the emphasis to a **real experience** of force. This aspect of consciousness had precisely the same status as the other inferences from experience. It was equivalent, for example, to the concept of 'matter'.

Lewes work represents an early union of the British school of experience with the more thorough-going empiricism that was gaining strength on the Continent. The tendency of this epistemological movement was two-fold: it insisted on the sensational origin of all concepts, and it asserted an operational criterion for the criticism of these categories. While this school accepted that notions like 'cause' and 'force' were referrable to aspects of experience, they were not prepared, *ipso facto*, to endorse the conventional usage of such terms. They were a school of empirio-criticism whose analysis of the utility of such concepts to science led to the drastic redefinition of 'matter' and 'force' and, on occasion, even to their virtual abolition.

Thus Ernst Mach rebelled in 1872 against the doctrine that the principle of the conservation of energy must rely on an atomic base. The kinetic theory was, he argued, an arbitrary addition of dubious value: heat might as well be regarded as a substance as motion, and no damage would be done

thereby to the central principles of conservation theory. Mach generalised this dissent from materialism in the philosophy of science that he developed in the 1870s and 1880s. In this, the dual emphasis upon experience as the ultimate given of scientific enquiry, and upon use as the standard for concept formulation and judgement, received its clearest expression. What was supposed to exist beyond experience was entirely a creature of the understanding, 'and has for us only the value of a *memoria technica* or formula, whose form, because it is arbitrary and irrelevant, varies very easily with the standpoint of our culture.'<sup>130</sup>

In mechanics, for example, the object of concern were the dynamic relations of phenomena. All concepts used in this discipline had to be adapted to the task of describing these events in the most comprehensive and economical manner; those which proved otiose should be discarded. Helmholtz himself had foreshadowed this attitude in his original paper 'On the Conservation of Force'. There he had stressed the centrality, and perhaps even the sufficiency, of space in the understanding of motion. He pointed out, as had Kant before him, that 'matter' was only known by its forces. However, these forces were not themselves ultimate categories: they 'depend in their action upon the conditions of space'. Helmholtz did not regard space as absolute, but as constituted by the events of themselves, and especially by the changing distribution of bodies. 'Motion' was the alteration in the 'conditions of space'; while 'force' 'can only be conceived of as referring to the relation of at least two material bodies towards each other; it is therefore to be defined as the endeavour of two masses to alter their relative position'.<sup>131</sup>

On this view, the problem of physical science was to refer the phenomena of motion to attractive and repulsive forces which were products of the spatial relations of bodies. 'Matter' existed only as the centre of such a force. These were very different conceptions of matter and force than the 'common sense' ones. Mach acknowledged this but refused to accept it as a valid criticism. The 'instinctive' outlook, although adequate for most purposes, was only one way of viewing phenomena; generally accepted notions should not restrict science in its search for concepts suited to more specialised needs. 'Force' in normal usage was an extension into the physical world of the experience of human activity; it must not, however, be reified into an entity independent of the mind nor should the common sense notion interfere with the scientist's search for a technically more serviceable definition.<sup>132</sup>

It was possible to extract from experience of motion a concept which made no reference to the



purely human conditions of movement. 'Force' on this view had no connection with will or effort or with any notion of 'efficient' causation. Instead, force could be fully described as 'any circumstances of which the consequence is motion'.<sup>133</sup>

One of the most extreme advocates of such empirio-criticism was the British mathematician W.K. Clifford. In his 1878 *Elements of Dynamics* Clifford espoused the definition of the aims and methods of mechanics outlined by Helmholtz. Just as 'Geometry teaches us about the sizes and shapes and distances of bodies, and about the relations which hold good between them, so Dynamic teaches us about the changes which take place in those sizes, distances, and shapes (which changes are called motions), and the circumstances under which motion takes place.'<sup>134</sup>

Mechanics was, therefore, the science of geometrical change. The conditions of such change, Clifford held, were the positions of other bodies, or the prior relations of space which existed in any system. The rate of this change was called 'force', and the generalisation which stated the circumstances under which motions changed was called the 'law of force'.<sup>135</sup> In other words, the term force was used as a summary of the manner in which the velocities of bodies were affected by their relative positions. The idea of force as efficient cause was entirely circumvented. Moreover, it was unnecessary to conceive of 'matter' as the condition of a force: the motion of a body was reducible to that of any point in that body.<sup>136</sup>

Clifford developed this approach in his later work, most of which was published posthumously. The only acceptable sense of 'force', he insisted, was as a symbol attached to the mutual influence that bodies had on one another: 'force is a function of situation'. The expression 'force exercised by a body' could be reformulated as 'the notion of something that depends on the situation of the body relative to the thing moved by its force'.<sup>137</sup> However, if force were thus reducible to other concepts, was it worth retaining?

Karl Pearson, who edited Clifford's papers, maintained that Clifford had been moving towards the expulsion of both force and matter from scientific terminology.<sup>138</sup> Pearson himself attacked the notion that a 'force' inhering in matter could be regarded as the cause of motion. He was equally critical of the suggestion that will was a cause. Both these hypotheses neglected the basic principles to which a scientific theory must conform: they pretended to provide explanations of phenomena when all that was possible was a description of them.<sup>139</sup>

Similarly, Pearson maintained that the idea of matter was redundant to the understanding of motion. In a system which contained only two bodies, P and Q, it was found that the ratio of their accelerations was constant. This ratio could be expressed by  $m$ , and it was conventional to call this term the 'mass' of the body Q. This conception of mass as the ratio of accelerations, Pearson held, was adequate to the scientist. However, because for bodies of the same substance mass tended to vary with volume, an 'indescribable something termed **matter** has been associated with bodies'. But this extra term added nothing to the operations of mechanics; it merely led to much philosophical and scientific confusion; it was best excluded.<sup>140</sup>

The invention of matter illustrated a general misconception about the scope of science. It was assumed that it was possible to say **why** velocity changed; that acceleration was somehow accomplished by force, whether that term was seen as independent or as a result of some property of matter. Science, Pearson maintained, could do no more than describe **how** events occurred: their inner mechanism was inscrutable. In this, Pearson certainly echoed Clifford's own views.

In 1870 Clifford gave a lecture at the Royal Institution on 'Theories of the Physical Forces' which began with the quotation from Goethe at the head of this chapter. Clifford held that scientific thought had passed through various stages at which different causes were identified at the bottom of things. At the moment, most scientist subscribed to the notion that force or energy was the basis of reality. However, Clifford foresaw that soon the vacuity of all such formulations would be recognised: science was not concerned with explaining why things happen; its task was to provide an 'exact knowledge of the facts'. It was enough, therefore, to view each phenomenon as an event; an occurrence for which no further explanation was either necessary or possible.<sup>141</sup>

In particular, the notion of causation needed to be redrawn. The current practice of mechanics assumed that time and space were continuous; however, physiology showed this to be a gratuitous assumption. All that we saw in motion was a series of discontinuous images which the imagination connected and improved. For all we knew, time and space might be discontinuous and an object in motion actually cease to exist for part of its supposed path and then reappear at a different point. In effect, just as Berkeley had argued that the idea of depth was not given in experience, so Clifford insisted that motion was a hypothetical construct only.<sup>142</sup>

Given the contingent status of this concept, there was no need to persist in the notion that the movement of bodies must consist in their passage along a continuum under the impetus of some 'force'. Clifford argued that a different mode of analysis was both possible and preferable. Motion should be regarded as a series of 'stills' at each of which a body possessed a set of properties in relation to others; from these properties it was possible to extrapolate its future condition at any subsequent point in time. The instantaneous position of a body was, therefore, a sufficient indication of all its future states; there was no need to suppose a 'cause' which carried it from one of these states to the next. Indeed, it was meaningless to ask why a body in such a condition at one moment and in a different one later: Clifford maintained that there 'is no why ..... the cause is only the fact that at some moment the thing is so'.<sup>142</sup>

In 1873 Clifford argued that all scientific concepts were amenable to similar treatment. The world was constituted by the brain processing, developing and building upon a highly tenuous flow of sensory information. This process of enhancement occurred according to certain 'rules', and these were 'the foundations of the pure sciences of Space and Motion'.<sup>143</sup> There was no necessity in any scientific concept. Even the fundamental ideas of mathematics, which Kant had invoked to refute Hume, were no exception. These were 'universal' only in the sense that the human brain was only equipped to perceive space, quantity, etc. in certain ways; the axioms of geometry consequently were 'statements about me and not about things in themselves'. The non-euclidean geometries of Lobatchewsky and Helmholtz showed that quite different universes could be conceived.<sup>144</sup>

Clifford was, therefore, the most 'radical' of empiricists. For him all knowledge consisted of experience supplemented by law; law, moreover, was merely contingent rules which had no necessary *a priori* status. As a result, they were subject to untrammelled revision; the standard by which all rules must be judged was their use in various human activities. Clifford declared his aim to be to bring science into a closer relation to the practical world. He argued that the 'subject of science is the human universe; that is to say, everything that is, or has been or may be related to man'.<sup>145</sup> His epistemological relativism had, therefore, a humanistic bent: man was not made for laws but laws were made for man. They were the tools whereby phenomena were classified to maximise the instrumental value of knowledge. Science was 'the guide of action; ..... the truth which it arrives at is ..... that which we may act upon

without fear'. Humanity progressed in proportion as it accepted and acted upon scientific truth.<sup>146</sup>

Ultimately, then, all knowledge was ethical knowledge: it laid down rules for action. However, Clifford implied that empirically well-founded knowledge was more suited to this role than any other. Just as *a priori* truths in science were to be rejected, so *a priori* moral maxims, founded supposedly upon the dictates of a transcendent being, were subject to scientific criticism and rejection. There was no moral authority higher than experience.

These issues were aired in 1877 when Clifford contributed to a symposium published in the *Nineteenth Century* on 'The Influence upon Morality of a Decline in Religious Belief'. While theists like Martineau insisted that belief in 'an ever-living and perfect Mind, supreme over the universe' was needed to invest morality with a universal character — to lift it 'from the provincial stage of human society to the imperishable theatre of all being'; agnostics like Clifford and Huxley insisted that moral constraints could operate independently of the idea of God.<sup>147</sup> Moreover, Clifford made it plain that it was not the **concept** of an 'external being' from whom moral law proceeded which outraged him, but the distribution of social authority with which this doctrine was habitually associated. He was against 'the practice of submitting human life to clerical control'. The true disaster to morality, he maintained, would come not from a decline in religious belief, but from a 'revival in any form of sacerdotal Christianity'.<sup>148</sup>

The negative import of naturalism in ethics was, therefore, to criticise the pretensions of the clergy. Empiricism, with its refusal to accept the absolute status of any belief, provided a powerful corrosive of theologically-based moral codes. But there was also a positive message in ethical naturalism: moral authority was transferred from one professional elite to another. While the cleric claimed to have access to the will of God for man, the scientist, and all those who claimed to have some brand of 'scientific' knowledge, were best equipped to advise upon what course of action was empirically most advisable. The latter group consisted of the 'new professionals' who became a force in British society in the later nineteenth century. This aspect of the naturalist strategy was most evident in Karl Pearson's advocacy of the rights of these 'brain workers' to a larger share in the direction of society.<sup>149</sup>

Pearson was highly critical of the spread of 'pseudo-science' in late Victorian Britain. By this he meant the kind of natural theology practised by Stewart and Tait which was extensively drawn upon by

philosophers and theologians hostile to naturalism. He regarded this movement as both intellectually and politically reactionary.<sup>150</sup> The first step of the reactionaries' strategy was to 'reconcile' science and religion — to show that the former's claims in no way invalidated the latter's. The next stage, was, 'as of old, to claim for religion a monopoly of the moral basis, and hence, by an easy paralogism, a monopoly of morality.'<sup>151</sup>

Pearson tried to meet these moves by an attack upon the epistemological assumptions of the reactionaries and by advancing a model of social order which showed naturalism to be an adequate creed to meet the moral as well as the material needs of society. In the former effort, he drew upon the empiricism of Clifford and Mach; in particular, he stressed the *instrumental* character of scientific concepts on this view. These symbols 'enable man to largely regulate his own future'.<sup>152</sup>

Knowledge, so understood, was power. It would enable a nation to achieve economic, military and cultural success. In the international competition for survival, those nations would do best which took steps to rearrange their institutions to enable the wielders of scientific knowledge the fullest scope. The instrumental rationality of 'brain workers' should, in effect, provide a 'national fulcrum' upon which society should turn.<sup>153</sup>

In his *Grammar of Science* (1892), Pearson had described how science could become a moral as well as a technical resource. He reasoned that in a democracy order must be achieved through the instilment of common principles of action; these principles had to be derived from 'a clear knowledge of facts, an appreciation of their sequence and relative significance'.<sup>154</sup> This, according to the empiricist was precisely what science supplied. By imparting a definite classification of facts, and the general notion of the regularity of their sequence, the scientist performed a social function which was quite independent of any metaphysical speculation. Indeed such speculation could only distract attention from the orderly sequence of phenomena to which notice should be drawn.

Science could supply an objective basis for political decisions; one evidently independent of all 'class bias'. Social institutions could be judged objectively by the criterion of whether or not they promoted the 'welfare of human society' or strengthened 'social stability'.<sup>155</sup> The scientific understanding of the world could have 'far more direct bearing on social problems than any theory of the state propounded by the philosophers from the days of Plato to those of Hegel'.<sup>156</sup>

Pearson recognised that such naturalism and all that it implied would meet resistance. The 'reactionary' interests of society would continue to argue that religion was indispensable to moral order because, thereby, they hoped to prevent the 'progressive' elements of society acquiring unqualified authority. In effect, there were two competing models of social control, each sustained by a different cosmology and by a particular conception of morality.<sup>157</sup> This condition was not peculiar to the 1890s, however: the competition between transcendental and naturalistic ethics had existed throughout the nineteenth century. Such 'moral' concerns conditioned the disputes about physical causation that occurred during this period.

# vi. The Sovereignty of God and the Freedom of Man

Whereas these two things are ..... the Fundamentals or Essentials of True Religion. First, that all things in the World do not float without a Head and Governor; but that there is a God, an Omnipresent Understanding Being, Presiding over all. Secondly, that this God being essentially good and Just, there is ..... something in its own Nature, Immutably and Eternally Just, and Unjust; and not by Arbitrary Will, Law and Command onely.<sup>158</sup>

(Ralph Cudworth)

The Right of Nature, whereby God reigneth over men, and punisheth those that break his Lawes is to be derived ..... from his Irresistable Power .....; and consequently it is from that Power, that the kingdome over men, and the Right of affecting men at his pleasure, belongeth Naturally to God Almighty; not as Creator, and Gracious; but as Omnipotent.<sup>159</sup>

(Thomas Hobbes)

The notion of 'natural law' compounds the ambiguities of two of the most radically under-determined terms in the language. The various interpretations which have been placed upon the idea can be distributed along two axes. The first of these has as its poles the concept of law as command and that of law as justice. The second reflects two versions of in what the authority of nature resides: one holds it to derive from a transcendent creator, the other to inhere in the world as such.

In the above quotation from Hobbes the concept of law as command is epitomised. The validity of law is held to reside solely in its origin in the will of God; men were obliged to obey it because it was a divine order and no other reason was necessary. In contrast, Cudworth held that the natural laws had a validity independent of their provenance: their justice could be appreciated without reference to the will of God. While the 'command' hypothesis holds that what God wills is right, the other maintains that God wills what is right. On the latter view, but not on the former, 'justice' has a content other than divine fiat.

These two views have habitually been associated with two strategies. The former has been the device of those who assert the rights of government and the need for subordination and order; the most striking example of this is Hobbes's own work. The theory that justice is independent of mere arbitrary will has been a tool for the critique of the existing order; in this case, 'nature' supplies a contrast with

the existing order, a set of ideals to pit against the actual. The difference between the two positions can be summarised as that between a conception of natural law as restrictive, as a vindication of control; and of natural law as a solvent of existing restrictions. In the first, natural law confirms human law: in the other it overrides it.

The source of law can also be held to be either immanent in or external to nature. As has been seen, this was a crucial issue of dispute among naturalists and their opponents in the nineteenth century. In the early and mid-Victorian period the location of authority outside nature tended to be the mark of a restrictive strategy; the incorporation of the regulative principle part of an assault upon the current distribution of power. However, as the century progressed, this generalisation became less valid. By the 1880s the issue between immanentists and transcendentalists had become that of what basis effective control required, not whether control was necessary.

Throughout the period, transcendentalists claimed to find a model for polity in the structure of nature, or rather in the subordination of nature to an external law-giver. It was as part of this argument that volitional theories of causation acquired their political significance. Thus Whewell wrote on moral as well as on physical philosophy; nor did he regard the two fields as separate. On the contrary, he held that there was a direct relation between them: the concept of **law** was crucial to this transition. Just as the laws of nature were the effect of divine volitions, so 'Virtue, which is conformable to the Supreme Law of our Nature is the **Will of God**, the Author of our Nature. Hence, the Law of Duty is the command of God.'<sup>160</sup>

The efficacy of moral beliefs relied upon their recognition as products of God's general government of the world. It was important to stress the **power** of God; only the dictates of a powerful being would command respect. The deity possessed 'an unbounded power to determine the Happiness or Misery of every one of us; He exercises this power so as to give a sanction to his Laws'. This message was the final import of 'Natural Religion'.<sup>161</sup>

In effect, God was the legislator for man as well as for matter. Man had free will, however, and could disobey divine law; thereby he invited retribution. Moral laws could be regarded 'as **Promises** to be made by God'. If men respected these injunctions, then eternal life and, perhaps, earthly delight would follow. But if they disobeyed God, the omnipotent ruler of nature, had ample means to inflict



punishment.<sup>162</sup>

Whewell went on to specify some of the laws which the 'Author of Nature' had laid down for his creatures. Both private property and the principle that the poor should work for their bread were among the dictates of providence.<sup>163</sup> In addition, there was a general duty to obey magistrates; governmental orders should be observed because 'the higher [civil] powers especially, are said to be of God: to resist them is to resist the ordinance of God, and to incur danger of damnation.' On this view, atheism, because it eroded the transcendental foundations of the state, was equivalent to sedition.<sup>164</sup>

These doctrines had a special application in the Britain of the 1840s. The 'Jacobin' menace had changed its complexion but it had not disappeared; instead it had taken on a still more threatening aspect. The social and economic changes of the early decades of the nineteenth century had produced a configuration of political forces which existing institutions could hardly contain. In the 1840s especially, when Chartism attracted massive working-class support, the danger of revolution was real. It was in response to such menaces to the social order, James Martineau wrote, that there arose a growing interest in 'moral and political studies' in the universities; Whewell's work was one example of this concern.

This type of moral theory should, in other words, be 'ascribed to causes social rather than academic'. Martineau proceeded to specify these causes: they included 'the rapid increase of large towns, the augmented power of capital and labour, the growth of our colonial empire, the altered proportion of sects'. These had 'started a number of social questions respecting the functions of government, the rights of industry, the means of public education, and the proper office of a church.'<sup>165</sup> The answer of the ancient universities to these questions was that of the groups that had the greatest stake in the preservation of the present order: the Established Church and the landed aristocracy.

As a result, the moral philosophy of Whewell and his like was an attempt to ground upon 'first principles' the 'moral order'; that is, it embodied a defence of existing institutions, and of the Conservative doctrines of obedience to authority as the indispensable condition of society. In Martineau's words, the ethical theory coming out of Oxford and Cambridge had an intensely practical orientation:

the ancient learning of the one, and the modern science of the other, are used, no longer

as the mere study of words and symbols, but as lessons in human nature and the Divine plan, as aids to the judging of living interests and duties.<sup>166</sup>

Especially in the Cambridge product, then, nature became a central resource in conservative ideology.

These ideas were passed down the line to become part of the rhetoric with which the 'masses' were bombarded. The way in which such principles were deployed polemically is illustrated by a sermon given by Joseph Goodsir in 1844. Goodsir, then a minister in the Church of Scotland, was a strong advocate of a volitional theory of causation. Through it, the study of nature would lead to 'that glorious God described from Newton' and to a model of reality which offered ample scope for moral instruction. Goodsir held that, wherever there was a 'multitude of beings', 'Rule must be upheld amongst them, that confusion may be avoided'. This was true of the material world: 'We know that in the natural world there are powers by which all things move, are guided and controlled'. Similarly, 'The Lord doth indeed reign in the moral world.'<sup>167</sup>

Thus in contemporary Britain, 'may be seen most pregnant proofs of the power and grace of our great King'; from him flowed 'our abundant blessings of wealth, of liberty, of social and civil good'. It followed that existing 'laws of social and civil order' were expressions of divine will; their transgression was therefore an offence against God's government as well as against that of man.<sup>168</sup>

Goodsir concluded that the 'view of the universe and of the Divine rule exercised over it which we have been taking, is assuredly fitted ..... to influence us most powerfully and advantageously.' Specifically, this cosmology inclined men to humility and deference, and to 'the deep conviction that each of us is subjected, along with every other creature ....., to most wise and beneficent laws'. In short, the recognition of God's power in the cosmos 'ought indefinitely to enforce the claims to obedience that our God and Saviour has upon us.' The result of neglect of these facts was 'sure destruction'.<sup>169</sup>

In this way, the notion that nature was determined by the divine will was turned to supply a normative social order. It became part of an ideology of hierarchy and control: a vindication of command in society in terms of the power of a transcendent governor to whose will all things could be referred. Such Newtonianism also had a more particular social message: it endowed those who claimed special knowledge of God's will with an exceptional cultural eminence.

The productions of the 'Cambridge Network' tended to be consonant with Coleridge's kind of

social philosophy. Whewell took over Coleridge's idea of the 'clerisy'; that is, of an elite who provided 'spiritual' guidance to society. This nebulous conception had a particular significance in the context of early and mid-nineteenth century Britain. As Martineau had noted, the social status of the Church was one of the outstanding political issues of the day. On the one hand were those who, as part of their general attack upon the forces of reaction, wished to see the disestablishment of the Church of England and the end of its integral position in the constitution. On the other, were the conservative interests which resisted such moves, and wished to retain the close bonds between Church and State.

Whewell, as a Cambridge cleric, was predictably among the latter. He held that, just as morality could not dispense with religion, so civil government must act in conjunction with ecclesiastical authority. A 'National Church' was the best equipped to fill this civic role.<sup>170</sup> Whewell argued that those who wished to separate Church and State threatened to detract from the spiritual character of the latter:

The State is an institution of Providence ..... The State also has its divinity, its sacredness; and is injuriously dealt with by the political philosopher, when it is treated as if it could never have anything to do with religion.<sup>171</sup>

In contrast, a political philosophy which recognised the need for a continued intimacy between Church and State upheld the divine sanction of earthly rule.

This line of argument also appeared in Martineau's writings; there too it was accompanied by a 'Newtonian' theory of causation. In many ways, Martineau's career paralleled Coleridge's. Both were Unitarian preachers of initially radical political and philosophical views, who subsequently became reactionaries in both fields. Their conversion from radicalism to conservatism involved the rejection of the empiricist heritage of Hartley, James Mill and Thomas Brown, and an insistence upon 'the rational necessity of an adequate spiritual cause for the cosmos, and the ethical experience of a superhuman Presence and Authority in the Conscience'.<sup>172</sup> To preserve the integrity of this being, Martineau, like Coleridge before him, assailed both the pantheistic attempt to assimilate God to nature and the empiricist effort to nullify the conception of his agency in nature.<sup>173</sup>

Martineau also developed Coleridge's notion of a 'clerisy'. However, in his version, the idea had a less sectarian implication than in Whewell's. Martineau was a minister of a dissenting sect and opposed

to the restriction of membership of the clerisy to ministers of the Established Church. Nonetheless, his basic conception of the social office of the clerisy was the same as Whewell's; he also invoked a similar cosmology to underpin this function. Martineau argued that the principle of 'subordination' was necessary to all forms of social organisation: this principle was inherent in 'the characteristics of man as a **moral** being'. Authority and government were thus indispensable; moreover, they needed to be provided with a transcendental basis. While human power might, for a while, suffice to command respect for law, soon 'invisible powers' had to be invoked to sustain moral order.<sup>174</sup>

Given this 'transcendent form of reverence', the social order could be justified by a higher authority: 'there is an invisible object of faith and homage distinct from the visible: the latter becomes simply the **representative** of the former, — the embodiment of a sacred rule over human life'. The Church was the chief agent for impressing this relation between the mundane and the sublime and, as such, an indispensable prop to government authority. In fact, in a perfect community, Martineau held, we would 'wholly sink the distinction between civil and ecclesiastical rule ..... a perfect coalescence takes place between the ideas of Religion and Government, and the rule of a Divine Law.'<sup>175</sup>

Since the Reformation, however, Church and State had tended to grow apart. It was all the more necessary to resist the tendency to relegate religion to private spirituality and to insist on its social duties. Church and State should perform complementary roles of social control, Martineau maintained: the Church was 'that system of organized agencies by which men in society may be led towards compliance **with the whole moral law**'; while the State could only make them abide by such parts of the moral law '**as are within the reach of public reward and punishment**'. In view of this cooperation there could be no objection to the existence of a State-funded 'National Church'.<sup>176</sup>

In effect, Martineau criticised both religious and political individualism. The former was represented by the 'Lutheran' doctrine of justification by faith; this implied that the individual had direct access to God and that religion was a matter of internal states and not of compliance to external ecclesiastical directions. The latter was little more than a secularised version of the same doctrine: liberal individualists like John Stuart Mill proposed 'to draw all reality and meaning into the inward life' and to make individual reason a sufficient basis for moral decision.<sup>177</sup>

In place of radical individualism, Martineau asserted a form of conservative organicism. The

individual could not generate his own moral goals because he had no criterion to distinguish between his various impulses. It was only in a larger whole that a realm of ends could appear; however, this also implied the **subordination** of the individual to the community. For obligation, Martineau insisted, 'we must have an authority ..... beyond self and higher than self'; moral imperatives had greater force in proportion as they seemed 'to reveal a Will greater than our own'.<sup>178</sup>

Organicism became during the later nineteenth century an increasingly important aspect of British political thought. Others emphasised, like Martineau, the notion of a divine authority and power which permeated both the natural and the moral worlds to which the human will should defer. Thus the Duke of Argyll wrote in 1867 that 'the phenomena of Nature are only really conceivable to us as ..... the expression of a Will enforcing itself with Power'. The idea of invariable sequence failed to capture the essence of the concept of causality: the notion of 'force' under the direction of mind must be added. In both the formation and in the regulation of nature, 'material forces have been always used as the instruments of Will.'<sup>179</sup>

Argyll was not a Tory of the old school; in fact, he belonged to the Liberal Party. However, he was a Whig rather than a Radical: although thoroughly imbued with the principles of political economy, he was no advocate of extreme reform. He held that all proposals for change must be checked against the 'laws of nature' which set limits upon what could be achieved. For example, it was part of God's purpose for man, as embodied in these laws, that wealth should be acquired and accumulated without undue constraint; Adam Smith was the great interpreter of the divine will in this matter. Moreover, there was another 'Law ..... in respect of Man which the Working Classes too often forgot, but which can neither be violated nor neglected with impunity'. Namely, 'the Law of inequality' which was 'one of the most fundamental fact of human nature.'<sup>180</sup>

The rule that the greatest rewards should go to the most 'gifted' was a conjunction 'which God has joined together, and which no man or combination of men have a right to put asunder.' Specifically, it was wrong for workers to combine in Trades Unions to try to enhance their relative economic position. Some amelioration was possible, but there was a limit to such levelling-out of inequalities 'in the nature of things in Natural Laws'.<sup>181</sup>

Argyll's work has a dual interest. It reveals the roots of the late-Victorian preoccupation with

'order' and 'authority'. An increasingly powerful and militant group threatened to upset existing institutions by its demands for a drastic redistribution of power and wealth; a rhetoric of community and social solidarity was one response to these growing signs of disunity and disorder. Secondly, Argyll's thought showed that, despite the overt hostility between naturalistic and transcendental cosmologies, they had much in common structurally and could be turned to the same ends. Both could invoke an extra-human arbiter to decide upon human affairs, be that arbiter God or nature. The decisions of these judges tended in the same direction: they both placed limits upon the potentials for social change.<sup>182</sup>

There was, however, a contrary tendency in Victorian thought which, although often nearly submerged, was never entirely suppressed, and which enjoyed a revival in the early twentieth century. This outlook refused the role of social arbiter either to God or to nature; it insisted that social morality could not depend on the mere dictates of any entity. Ethics were not on that account merely subjective: there were moral principles that could be derived from a rational appreciation of man and society. These were 'laws of nature', not in the sense of statements of the actual and necessary, but in embodying the idea of a better society in which human potential would be fulfilled more completely.

John Stuart Mill was the outstanding spokesman of this philosophy in Victorian Britain. From the outset of his career he criticised the rival view that the moral could be reduced to the injunctions of some being. Mill attacked Adam Sedgwick, the Cambridge geologist, in 1835 for defining morality as 'a mere index of the will of God'. Mill held that if virtue was such only because God commanded it, 'if it derives all its obligatory force from his will — there remains no ground for obeying him except his power'. On this theory, coercion was the basis of all order.<sup>183</sup>

Such arguments, Mill considered, were features of Sedgwick's cultural matrix — Cambridge University — and typical of his social group — the clergy. In general, Mill argued, when the clergy pronounced upon philosophical questions, they did so to support 'established opinions' and to obstruct progress. Thus the essence of Whewell's moral theory was to support the existing order: 'we find him everywhere inculcating, as one of the most sacred duties, reverence for superiors, ..... and obedience to existing laws'. In this scheme, 'law', in the sense of command, was the only source of right and

obligation.<sup>184</sup>

Mill held that these views and their associated cosmology and epistemology were both irrational and reactionary. The one followed from the other since the ideology of radicalism insisted that the exercise of unfettered reason must lead to 'progressive' political opinions as well as to a 'scientific' world-view.<sup>185</sup> The 'rational' or 'scientific' outlook had two main elements: an empiricist epistemology and an insistence that man should be regarded as an integral part of nature. This contrasted with the two main pillars of the irrational reactionary cosmology: intuitionism and the doctrine of the existence of a spiritual world to which the more important part of man's being belonged.

Other radicals, like John Morley, were most concerned to undermine the latter position by showing nature to be a self-contained unity into which no spiritual agency could intervene. Mill himself contributed to the repudiation of a *priorism*; he saw this theoretical exercise in essentially political terms, arguing that the 'notion that truths external to the mind may be known by intuition or consciousness, independently of observation and experience is ....., in these times, the great intellectual support of false doctrines and bad institutions.' Appeal to 'intuition' was another instance of the reference to authority, rather than reason as the basis of judgment; it enabled existing practices to be mystified and placed above criticism. The chief strength of the doctrine in morals and politics, Mill went on, 'lies in the appeal which it is accustomed to make to the evidence of mathematics and of cognate branches of physical science.'<sup>186</sup>

In consequence, Mill's *Logic* was at once a refutation of a philosophical and a political position: Whewell the moralist and the Tory as much as Whewell the metaphysician was its target, and Whewell was important only because his work represented the attitude and ideology of the forces of reaction in British society. There was a crying need for a book like the *Logic* in contemporary Britain, Mill told Hippolyte Taine in 1861, when there were twenty *a priori* and spiritualist philosophers for every empiricist.<sup>187</sup> Mill had met their socially dangerous doctrines 'on grounds which they had deemed unassailable'; namely, the foundations of scientific knowledge. He had shown that experience was an adequate basis for thought here; it followed that there could be no higher criterion than empirical consequences for judging political issues.<sup>188</sup>

An instance of the political use to which intuitionism had been put was found in the 'absolutely

loathsome' theism of Henry Mansel.<sup>189</sup> In his 1858 Bampton Lectures Mansel had argued for the necessary **dependence** of the human mind upon a 'Superior Power'. The human mind could not aspire to understand the divine wisdom, still less to question it; it should, therefore, passively obey God's injunctions.<sup>190</sup> Mill considered this call to passive obedience in the face of a higher power 'simply the most morally pernicious doctrine now current'. According to Mansel, there was no other standard of right than what God willed; what might **seem** evil might still be good in God's eyes. On such principles, the grossest injustices could be justified and any issue put beyond the reach of rational appraisal.<sup>191</sup>

Mill objected so strongly to Mansel's ethics because they impinged on two of the major concerns of his own political thought. Mansel advocated a passive attitude to what was given by tradition and 'intuition'. This was an aspect of the conservative political philosophy which viewed a society's institutions as given by fate rather than adapted to current needs. Accordingly, politics consisted in the discovery of the 'natural properties' of institutions and in accepting these as final limits of action. Mill's response was to contend that society was 'given' only in the sense of being the original materials upon which politics should work to make institutions more amenable to the community's wants.<sup>192</sup>

In addition, Mill held that the idea of the 'dependence' of man upon a transcendent being was a feature of an outmoded authoritarian and hierarchical political dogma. In particular, it was assumed that the 'labouring classes' were incapable of 'self-determination'; their lives should be 'regulated for them'. The 'duty' of performing this regulation fell upon the 'higher classes' whose authority the lower should respect.<sup>193</sup> Mill, in contrast, argued that to 'be under the power of some one ..... is now, speaking generally, the only situation which exposes to grievous wrong.' He insisted upon the right of the working-class to self-determination, arguing that 'it is their own will, their own suggestions, to which they will demand that effect **be** given , and not rules laid down by other people.'<sup>194</sup>

In consequence, Mill was hostile to all attempts to erect an external authority to regulate society. In one of his last works he discussed the way in which 'nature' and its associated terms had become a central resource in efforts to subordinate the individual to some higher power. Reactionaries had argued that 'the word Nature affords some external criterion of what we should do'; in fact, Mill asserted,



nature was a source of **means** not of ends. It was the nidus of potential, not of restriction. While the former view was necessarily reactionary, the latter was the proper concomitant of a progressive political attitude.<sup>195</sup>

Mill was especially concerned about conservatives' appeals to a 'Creator's will' in contesting change. However, although such appeals certainly continued into the late nineteenth century, they tended to be superseded by another brand of conservative polemic. Nature itself, even when ostentatiously shorn of all divine influence, was invoked as the foundation of society and as the cause of certain 'necessary' features of social structure. The location of authority was thus shifted and the personnel of the 'clerisy' revised to consist of the wielders of scientific rather than theological knowledge. However, the **strategy** of such naturalism was identical with that of the transcendentalists: 'God' and 'nature' served equally to discipline and to control.

## CHAPTER THREE: The Physical Basis of Life

### Introduction

British life science in the first half of the nineteenth century was an amorphous activity. Questions which today would be classed as physiological, anatomical and developmental were conflated, and there was a corresponding lack of a clear appreciation of the need for separate disciplines and competences for the pursuit of such enquiries. In effect, by all the usual standards, biology was 'pre-professional' at this time. By 1900, in contrast, there was a far stronger sense of the internal diversity of biological knowledge, as well as a number of 'schools', each with its own methods and assumptions, to cultivate each specialism.

The move to professionalism was most striking in the case of physiology. In the early nineteenth century the term did not possess its modern signification; instead, it comprehended all forms of investigation of the body, but especially those of some medical relevance. The physiological was not distinguished from the anatomical: on the contrary, one powerful tendency of thought in Britain at this time based its doctrine upon the premise that function **must** be studied in relation to structure. Between 1840 and 1880 the two fields were drawn apart, each gradually acquiring its own conceptual and material resources and ceasing to depend on the other for either. This transition to a science of form is considered in Chapter Five. The aim of this chapter is to describe the making of a science of function.

The account has two main stages. First, the creation of a school of 'physiological anatomy' chiefly in Edinburgh and London will be discussed. This was the prelude to the establishment of a partly complementary, partly rival, experimental physiology in the latter city. Both these approaches to the body operated with a distinctive model of the organism. Both were physicalist in that they identified life as a property of material forces; but whereas physiological anatomy regarded life as a property of **organisation**, the experimentalists stressed its dependence on **substance**.

A third philosophy of life, or rather a congerie of life theories, must also be considered. Vitalism was a significant doctrine in Britain throughout the nineteenth century; its adherents existed both among the structuralists and the substantialists, and among those who subscribed to neither theory. Vitalism was less an ideology of professionalisation and more a token of an older notion of the cultural import of natural knowledge. <sup>S</sup> <sub>A</sub> Despite the development of a professional science of biology in Britain, concepts of life continued to fulfil this wider role until the early twentieth century.

### i. Old Vitalism

All discussions of vitalism must begin with a recognition that the term contains a multitude of sins.<sup>1</sup>

In general, vitalism involves the claim that organic phenomena are so special that they cannot be viewed in the same terms as inorganic nature. It usually 'follows' that some peculiar 'vital principle' needs to be invoked to explain these divergences from physico-chemical norms. However consensus among vitalists ends when it comes to defining what the vital principle is and to deciding what counts as an explanation of a vital process.

On the one hand is the 'sceptical' or 'methodological' vitalism espoused by Blumenbach, at least in his programmatic writings. Blumenbach acknowledged that organic phenomena such as ontogeny were beyond the scope of the mechanistic explanation which was supposedly appropriate to physical science. Certain broad groups of events could, however, be distinguished as closely connected. This connection could be expressed in terms of having the same 'cause', and to that cause the name 'vital principle' was given. Thus Blumenbach distinguished a **nisus formativus** or **Bildungstrieb** to whose action development could be attributed. He did not attempt to say how this principle operated, claiming that he used the expression 'nisus formativus, merely to distinguish it from the other orders of vital powers, and by no means to explain the cause of generation'.<sup>2</sup>

In this sense, a vital force was merely a 'heuristic device for unifying phenomena where the laws of mechanical causality do not suffice'.<sup>3</sup> It was at once a confession of ignorance and a methodological recommendation: although the inner character of the vital power was unknown, the identification of the field of its action directed attention to the interrelations between phenomena. 'Explanation', in the mechanistic sense, might be impossible, but it was open to the investigator to describe the working of the vital principle as it affected visible entities. Blumenbach himself detailed several 'laws' of growth which purported to show the manner in which the **Bildungstrieb** worked.

However, such methodological asceticism proved difficult to sustain in practice. Among Blumenbach's disciples in the early nineteenth century there was a tendency to the reification of what had been proposed as only a regulative principle: in their work 'vital forces became agents in the arrangement of living matter; what had been intended as a heuristic principle assumed a real existence

and material activity.<sup>4</sup>

The nature of this entity remained open to dispute. Many vitalists of this school stressed the 'teleological' character of the *nisus*: its tendency to pursue, as if purposefully, a given end. In this they harked back to the animistic vitalism of Stahl, and suggested, more or less explicitly, that the vital principle had psychic characteristics.<sup>5</sup> Others rejected this option and preferred to regard the vital principle as a peculiar kind of matter. These often cited John Hunter's physiology as an authority for their position.<sup>6</sup> The distinction between these two positions, however, was far from absolute.

In Britain in the first half of the nineteenth century all of these varieties of vitalism existed. Their distribution was not random; rather it conformed to the different interests in the body that were present in contemporary society.

William Lawrence, for example, in his 1819 lectures on physiology denied that vital functions could 'be in the slightest degree elucidated by mechanics'.<sup>7</sup> Chemical and physical forces were continually being modified by the action of vital principles; in consequence, a reductionist programme was futile. The 'main springs of animal functions' belonged 'peculiarly and exclusively to living organic textures'.<sup>8</sup> In effect, Lawrence espoused the 'positive vitalism' of Bichat: that is, he held that although vital properties were irreducible to any other form of power, it was possible to localise them in particular organs and tissues. There was a constant concomitance between function and structure; physiological explanation consisted in establishing that relation and in detailing its histological base.<sup>9</sup>

Lawrence was aware of another vitalism which supposed the presence in the body of 'an immaterial principle' or a 'material, but invisible and very subtle agent', which was 'superadded to the obvious structure of the body, and enables it to exhibit vital properties'. He regarded such theories as useless as explanatory tools:

The former explanation will be of use to those who are conversant with immaterial beings, and who understand how they are connected with and act on matter. But I know no description of persons likely to benefit by the latter: for subtle matter is still matter; and if this fine stuff can possess vital properties, surely they may reside in a fabric which differs only in being a little coarser.<sup>10</sup>

Lawrence's concern was to establish a sphere of purely physiological discourse unpolluted by philosophical or-theological considerations. He emphasised, for instance, the irrelevance of physiological evidence to the theological question of the existence of the soul; that rested 'on a species of proof altogether different ..... An immaterial and spiritual being could not have been discovered among the blood and filth of the dissecting room'.<sup>11</sup> The continuing involvement of physiology in such affairs could only distract it from its proper concern with medically valuable knowledge. It was by this criterion that Lawrence discriminated between the varieties of vitalism. 'Positive' vitalism, as Bichat had demonstrated, was eminently suited to the concerns of the physician; while animism and the doctrine of a subtle fluid were at best worthless and at worst dangerous.<sup>12</sup>

However, as Lawrence prophetically remarked, the 'priests of former times used to rain a torrent of abusive epithets, as heretic, infidel, atheist, and Lord knows what, on all who had the audacity to differ from them in this opinion.'<sup>13</sup> Nor had this 'ecclesiastical artillery' been silenced; on the contrary, it rained a particularly heavy barrage down upon Lawrence and upon all who occupied his position on vitality.<sup>14</sup> Such vitalism, it was argued, was no more than materialism; a quite different understanding of life was needed to satisfy the requirements of theological orthodoxy.

This view, as articulated by John Abernethy in 1815, had both a positive and a negative element. The latter involved the insistence that life was ultimately independent of organisation; it was, for example, possible to speak of the power of irritability without reference to the fibrous structure of the muscles because the property was not invariably associated with such an organisation.<sup>15</sup> The positive aspect of the doctrine was the claim that the true seat of vitality was that proposed by Hunter; namely, some 'some subtile, mobile, invisible substance, superadded to the evident structure of the muscles, or other forms of vegetable and animal matter.'<sup>16</sup>

The identity of this subtle substance tended to fluctuate. At times, it was described as a natural entity akin to the 'fluids' which had been hypothesised to explain electrical and magnetic phenomena. In this sense, the vital principle was not an 'imaginary thing' but 'a substance perceptible by the thermometer ..... its phenomena are as capable of arrangement and investigation as any other series of physical facts.'<sup>17</sup>

On the other hand, Abernethy's own account of the vital principle made it appear less a

substance than an agent. He alleged that life was the 'great chemist' at work in nutrition; the 'architect' that designed and built structure; and the source of the regulation of those functions that were not controlled by the brain. On this theory, the subtle fluid was the instrument whereby 'Life' did all this.<sup>18</sup>

Abernethy had a particular polemical object in maintaining this view of vitality. Namely, to discredit the 'Modern Sceptics' who insisted that life should be considered 'a property of certain structures, as gravitation and elasticity are said to be the properties of matter'. To these sceptics the notion of life as something superadded to matter was anathema, because they perceived that 'the superaddition of life to structure may ..... warrant the supposition of a substance having the properties of perception being superadded to life; and that there may be "more things in heaven and earth, than in their philosophy dreamt of." '19

In short, Abernethy and his allies argued that the strategy of the sceptics, among whom (despite his denials) they numbered Lawrence, was to collapse several crucial distinctions. Christianity, Thomas Rennell claimed in 1819, was in danger from an infidelity which was making its 'alarming way' through 'the lower departments of the law, medicine, and of the counting houses'. The scientists and medical men who subscribed to 'French' philosophy were especially guilty, because they ignored the existence of God's power in nature and would account for all phenomena on purely naturalistic grounds.

The organism was a central case in point. By propagating Bichat's view of vitality the sceptics sought to undermine the boundary between spirit and matter. Their aim, Rennell declared, in advocating that life was dependent upon organisation was to challenge 'the immortality of the soul, and with it every thing that distinguishes man from the grass upon which he treads'; by confusing life and organisation they threatened to confound 'the body and the soul, the material and the thinking principle'.<sup>20</sup>

Apart from Lawrence the main British target of such attacks was T.C. Morgan, who, in 1819, had published an outspoken criticism of the 'deplorable degradation of metaphysical science which followed its removal from the grasp of natural philosophers and its amalgamation with scholastic divinity'.<sup>21</sup> This was much more an overtly political document than Lawrence's utterances; Morgan

was a true 'Jacobin', who embodied the connection between 'French' physiology and French political philosophy which Abernethy and others had condemned. Unlike Lawrence, Morgan did not deny a moral role for physiological knowledge; indeed, he insisted that morality should be founded on naturalistic grounds from which all appeals to a transcendental authority had been purged. It was only as a 'branch of the natural history of man' that ethics could be successfully treated: good and evil, Morgan claimed, 'are principles intelligible only as they relate to the law of organic existence'.<sup>22</sup> His authority for this assertion was the *Ideologue* Cabanis.

Such a moral code required the erosion of the barriers which had been erected between man and animals, and between the living and the non-living. All of these formed part of a continuous chain of being in which gradual transition rather than sudden jumps was the rule. The most important of the false distinctions to be rejected was that between matter and spirit. It was, Morgan argued, a primitive superstition to suppose that 'the great phenomena of nature' were the work of spiritual agents. The immediate consequence of this view of nature was the doctrine that

the visible and tangible species owe their forms to and properties to the interference of such intelligences, and that the matter of which they are formed is of itself inert, motionless, and incapable ..... Under this bias, existences were divided into two classes, material and spiritual: and ..... mankind still continues to stickle for the distinction, and what is worse make it a watchword for fanaticism and persecution.<sup>23</sup>

The theory of 'vital powers' was merely one aspect of this kind of ontology. If matter were, *ex hypothesi*, passive, then an immaterial principle must be found to explain its movements. The various subtle fluids contrived to fill this role were, Morgan held, mere prevarications: a 'little attention' sufficed to show that the vital principles were only 'philosophical creations, middle terms between matter and spirit'. The invention of an 'immaterial substance' to house these powers, he continued, was 'but the placing of the elephant upon the tortoise. They must either be taken as an active species of matter, or recourse must be had to the spiritual system'.<sup>24</sup>

While Abernethy favoured the latter alternative, Morgan maintained that 'Nothing is strictly passive in nature'. In place of the notion that spirit in the organism and, by extension, in the universe generally, was the cause of all change, he advanced the view that 'It is by making use of the inherent

properties of the several species of matter, and not by thwarting or controlling them, that man effects his operations.'<sup>25</sup>

It was to the theological implications of these claims that Abernethy and Rennell reacted. In this case, the microcosm of the body directly mirrored the macrocosm of nature. If spiritual forces were sovereign in the one, then their rule was assured in the other. But, 'once we have argued ourselves out of the existence of our soul, which is a spirit, by the very same process we argue ourselves out of the Almighty, who is spirit also.' The most effective response to such a dangerous cosmological revolution, Rennell concluded, was a vitalism like Abernethy's.<sup>26</sup>

The subtle matter of Hunter afforded the buffer between spirit and matter which the modern sceptics threatened. Life was not organisation; nor was it spirit. Each was a separate element in man and it was an error to confuse them. But life, spirit and body were not of equal dignity: matter was the basest of the three, and life was an imperfect shadow and a tool of spirit. The vital principle served, therefore, to defend both the distinction between spirit and matter and the hierarchy between the two which Lawrence and Morgan threatened.<sup>27</sup>

The fullest statement of such vitalism was made by John Barclay in 1822. Barclay, head of the Edinburgh anatomy school, set out to integrate this view of the organism into a general cosmological system, and thereby to discomfit the materialists. Like Rennell, Barclay linked the notion that life depended upon organisation to the heresy that mind was similarly a consequence of cerebral structure. A large part of his work was devoted to an attack upon the phrenological school which argued exactly this point.<sup>28</sup> It was the moral consequences of the doctrine which concerned him. On this hypothesis, Barclay argued, 'human existence terminates with death, schemes of expediency and self-interest take place of religion and moral obligations, and thoughts and actions however criminal are ..... to be held disgraceful only if detected'.<sup>29</sup> If there were no need to fear future judgment, why should men obey the law of God?

Moreover, when extended to nature as a whole, such opinions threatened the very survival of God as a transcendent law-giver. If atoms, acting according to their inherent powers could combine to construct a human organism,

and exhibit the phenomena of a human soul, they may, when connected in greater numbers,



produce a deity; and thus if a deity be thought necessary to account for the various phenomena of the universe, he must be subjected to matter and motion, and to the eternal and immutable laws of fate and necessity, in such a way that if he owe not any thing to man, neither can man owe any thing to him.<sup>30</sup>

In other words, the effect of a confusion of spirit and matter would be the effective abolition of God as the sovereign of nature, who had imposed moral laws on man just as he had imposed physical laws on matter. The resultant 'God' would cease to be the being who 'laid the foundations of rewards and punishment for men', and would become indistinguishable from nature.<sup>31</sup>

While Abernethy had been chiefly concerned to protect the autonomy of the human soul, Barclay undertook to defend the

idea of an eternal being, possessed of unlimited power and intelligence, who watches over the affairs of men, continues their existence beyond the grave, and who, so far as they have a control over their conduct, is to render them accountable for their motives and actions.<sup>32</sup>

However, both men employed essentially similar resources.

Barclay reviewed and rejected Blumenbach's version of vitalism as unsuited to his purpose. Blumenbach had invoked vital powers without specifying their psychic character; moreover, he had neglected to name the source of such power. Barclay denied that such an empty device explained anything: 'Does it amount to more than a truism to say, that a plant or an animal is formed by the power that formed it?'<sup>33</sup> Instead, he looked to a British tradition of natural philosophy to supply an adequate conception of life. The seventeenth century Cambridge Platonist, Richard Cudworth, had posited a 'species of plastic nature..... a real but incorporeal substance, through the medium of which the deity organizes animals and plants'.<sup>34</sup> This concept had been taken up by Newton who had defined its theological implications still more closely.

Newton had complemented his notion of the inertia of brute matter with that of a subtle ethereal medium by means of which force was propagated through the universe. Newton had not, however, attributed ultimate motive power to this ether, nor to the active principles which were somehow embodied in it. He had held that the phenomena of life, as of all motion, were 'the effect of nothing else than the wisdom of a powerful ever-living agent'.<sup>35</sup> Newton, therefore, had not

hesitated to name the cause of life and action in nature; on the contrary he had held that the order of both the organic and of the inorganic universe should be seen as 'proofs irrefragable that the first cause must be a being incorporeal, living, intelligent, omnipresent, who sees all things and directs all things by his wisdom and power'.<sup>36</sup>

The organism, on this view, was an especially conspicuous theatre where the divine will operated through the agency of subtle, semi-spiritual, vital principles. Barclay did not attempt to detail these principles, nor to elucidate the relation of the divine management of the body to that carried out by the human will. Such questions were irrelevant to his main goals: to sustain the matter-spirit hierarchy which the materialistic physiologies of Priestley and Erasmus Darwin in the last century, and those of Lawrence and Bichat in the present one, had threatened.

Barclay gave some indication of the circumstances which had generated such a concern to maintain the power and independence of God. He claimed that the view of life that he had advocated was native to human instinct; but noted that others had ascribed another source to it. Certain 'atheists' had argued that such vitalism was 'continued to serve the purposes of priests and politicians'. Their own view of life was as politically interested: they attributed all power to nature and then asked 'where is there any occasion for the God whom priests and tyrants would impose?'.<sup>37</sup>

Such utterances need to be placed in their contexts before their full significance is apparent. Both London and Edinburgh were between 1815 and 1848 thriving centres of radical activity; not since the heady days of the 1650s had there been such a ferment of agitation against the established order. This 'Jacobinism' encompassed several movements composed principally of bourgeois, artisan and professional elements. The mainspring of their activity was resentment at the 'Old Corruption' embodied in the present regime; in particular they complained at the mass of restrictions and exactions which enabled aristocratic and clerical elites to control political and economic institutions and to milk other sections of society for their own upkeep.<sup>38</sup>

Radical rhetoric was, in consequence, largely a critique of these impositions. At its most elaborate this comprised the atomistic political theory of Bentham and the libertarian economics of Ricardo, in which 'utility' or individual rationality, was preferred to tradition and authority as a criterion of social organisation.<sup>39</sup> At its most basic, such polemic consisted of a denunciation of all

infringement of personal liberty and property, whether by Church or by State.

One aspect of radical ideology was a denunciation of the moral foundations of the existing order: that is, of the metaphysical and cosmological doctrines which had been raised to justify the existing distribution of power. Jacobinism was often identified with atheism because this moral order was intimately related to the Christian world-view, and especially to the related dogmas of divine government and providence in the universe and of the immortality of the soul and judgment after death. The existing order, conservatives argued, had divine sanction, and God had the means to enforce his will both in this world and in the next. The radicals repudiated this cosmology and insisted that man be seen as a natural being, independent of any transcendent authority, whose destiny was confined to his earthly existence. Materialism was the cosmological basis of this position. As the radical ideologue Richard Carlile put it,

Instead of viewing ourselves as the particular objects of the care of a great Deity, or of receiving those dogmas of the priest which teach us that every thing has been made for the convenience and use of man, and that man has been made in the express image of the Deity, we should consider ourselves but as atoms of organized matter.<sup>40</sup>

In contrast to Barclay, Carlile denounced the legacy of Newton: in particular, he criticised Newton's reduction of matter to a bare essence devoid of all active properties. That was the doctrine of priests and politicians. Newton himself had been 'in the employ of the court, and consequently under the trammels of Kingcraft' and had brought to nature 'a conviction that there was such a god or such gods as Priests had taught him, and he endeavoured to make all scientific researches subservient to it.'<sup>41</sup> As a result of this political and theological bias, Newton's cosmology had been replete with metaphors of authority and regulation; Carlile quoted Newton's own declaration that 'The Supreme Being governs all things, not as the soul of the world, but as Lord of the Universe: and upon account of his dominion, he is styled the Lord God, Supreme over all.'<sup>42</sup> The means of this dominion were the active principles which mediated between spirit and matter whereby God exerted his will.

Carlile welcomed all attempts to break down this hierarchy and the model of control it implied. He singled out Lawrence's lectures as an especially important assault upon the dogmas of 'priestcraft'; in general, he argued, 'the Medical and Surgical professions' were the most prolific providers of resources

for the materialist.<sup>43</sup> Despite Lawrence's protests, it was in this light too that the defenders of the old corruption, such as Abernethy and Barclay, saw his doctrine of life. Such a model of the organism, Rennell warned, was a means of undermining social deference and the structure of command: if there were no effective God, then 'man is his own master, responsible neither to his Maker — who is nobody; nor to his soul — which is nothing'.<sup>44</sup>

On the other hand, the doctrine that the body was dependent upon a vital force for its action, and that this force was an instrument of spirit, upheld both the sovereignty of God and the existence of the soul. Thereby, the moral hierarchy was also conserved. Lawrence recognised that this concern lay behind the doctrine of a subtle life-giving fluid which he had rejected:

this subtle and mobile fluid is brought forward with more ambitious pretensions [than purely physiological ones]; and it is not only designed to show the nature and operation of the cause, by which vital phenomena are produced, but to add a new sanction to the great principles of morals and religion ..... An obscure hypothesis ..... is to make us all good and virtuous, to impose a restraint upon vice stronger than Bow Street or the Old Bailey can apply.<sup>45</sup>

In short, such life theories were but one aspect of the repressive conservative reaction to the threat of political upheaval.

The strength of the interest which underlay vitalism of this variety was such that the doctrine assumed the status of orthodoxy in the natural theology of the next two decades. Deviations from it met violent condemnation. Charles Bell, in his 1836 edition of Paley's **Natural Theology**, dismissed accounts of ontogeny which attempted to explain the phenomena of growth mechanistically without reference to an immaterial vital principle.<sup>46</sup> Similarly, William Prout in his 1834 Bridgewater Treatise declared that 'within a plant or animal, there exists a principle, or agent, superior to those whose operations we witness in the inorganic world'; this agency he identified with 'the Great Architect of the Universe'.<sup>47</sup>

But it was William Kirby who stated the political significance of vitalism most clearly. Kirby wrote in the idiom of the Hutchinsonian theology of the previous century; in consequence, his work was full of symbolic imagery and Biblical references. Unlike his Hutchinsonian forebears, however,

Kirby did not balk at involving divine power intimately in the workings of nature. He held that scripture showed that not only had God created the universe, but that he remained active in its processes: 'Upholding all things by the word of his power'.<sup>48</sup> The great error of Lamarck and of other materialists who attributed all potency to body, Kirby contended, was that 'he excludes the Deity from the government of the world that he has created putting nature in his place'.<sup>49</sup> Ultimately, God was the source of all physical power. But he did not act upon matter directly: there were 'inter-agents between God and the material world, by which he acts upon it, and as it were takes hold of it; by which he has commenced and still maintains motion in it and its parts'. Various names had been given to these instruments, including Cudworth's 'plastic nature' and Newton's ether; all, however, expressed or implied 'an agency between the Deity and the visible world, directed by him'.<sup>50</sup>

Vital forces were one species of such agents, which, together, constituted a finely graduated scale proceeding from God to all aspects of his creation. Nor was there any basic difference between the social and material worlds in this respect. The powers that ruled under God did so both in the physical universe and, 'with regard to our own planet, have power in his church, or over his people'.<sup>51</sup> In effect, social institutions, like the state and the church, were only parts of the natural order that God had originally created through the *Logos*, and which he now maintained by the operation of active principles. Kirby expatiated upon the political implications of this world-view in language reminiscent of Newton's. Saint Paul, he held, when describing the creation of the world by the *Logos*,

mentions particularly **four** ruling powers in nature and grace — **Thrones, dominions, principalities, and powers**. This may be interpreted of all rule and government in both heaven and upon earth; which is all derived from Christ, as King of Kings and Lord of Lords, to whom **all power is given in heaven and earth**: who therefore is the insessor of the cherubim [ie. active principles], acting by all the powers that he hath created, whether physical or metaphysical, whether civil, ecclesiastical, or spiritual; **for He upholdeth all things by the word of his power**.<sup>52</sup>

Nature, therefore, implied a command structure which extended into society. Just as God (in one or other of his persons) ruled the universe through a series of quasi-spiritual instruments; so he ordered society through his appointed deputies. Just as physical power was, in the last analysis, divine power; so

political and ecclesiastical authority derived from God. By implication, the existing order was endowed with a transcendental sanction and hierarchy was identified as a necessary feature of the cosmic order. It was for this reason that natural theologians strove against those who would drive spiritual agency out of nature; it was why they tried to bring the deity 'nearer and nearer to us, that we may see and acknowledge Him every where, as the main-spring of the universe, which animates ..... and upholds it in all its parts and motions ..... Maintaining his own laws by his own universal action ..... and by his cherubim of glory'.<sup>53</sup>

The analogies between the contexts in which natural philosophy was formulated and propagated in Britain in the 1650s and in the 1830s are extensive. In both cases, a crucial fact was the existence of a radical urban sub-culture which attacked social hierarchies by denying the absolute distinctions which were supposed to obtain between spirit and matter in nature and the dependence of the latter on the former: the 'radical cosmology' held that matter was sufficient to explain all phenomena. In both cases, the hylozoists were met with a party of order who sought support from natural philosophy for the dichotomies of society. However, while in the earlier period, the controversy tended to centre upon the character of physical agency; in the latter there was greater interest in questions of vital action. But the structure of the debates remained virtually identical.<sup>54</sup>

As a result, the philosophy of the organism became a major battleground between competing factions in the early nineteenth century. The forms and properties of the body signified the distribution of power in nature, and this, in turn, implied a normative order for society. To pronounce on the nature of life was, therefore, to invite a political construction; Lawrence's pleas for the autonomy of physiological language went unheeded. Far from being a discrete field with its own goals and concepts, physiology was embroiled in the broader interests which shaped the cultural productions of the time. Specifically, a particular form of vitalism, which saw the body as dependent on the action of an immaterial principle, was endowed with the status of the politically 'safe' view, while others were actively discouraged as dangerous.

In one respect, however, the nineteenth century context differed from that of the earlier period. This difference resided in the existence within society of a more specialised interest in physiological questions. Although it would be anachronistic to speak of a 'physiological profession' in Britain during

the first half of the nineteenth century, there was a body of workers attached chiefly to the medical schools who had an occupational concern in the organism. They looked to conceptions of the body to supply them with research strategies and programmes of pedagogic practice. In general, these men found little of worth in the vitalism of Abernethy and his kind. On the other hand, the 'French' philosophy favoured by Lawrence seemed to offer promising lines of enquiry. Therefore, they looked to the Continent, first to France then to Germany, to provide models of vitality suited to their purposes.

## ii. The Fabric of Life

The doctrine of the vital principle as a subtle fluid through which spirit controlled matter never enjoyed unchallenged acceptance. Although this vitalism received one of its fullest expressions from John Barclay, a head of the Edinburgh anatomy school, that institution, and the Edinburgh medical community more generally, provided a major source of dissent and of alternative life theories.

As early as 1829, James Prichard, one Edinburgh product, insisted on separating the **theological** question of the existence of the soul from the **physiological** issue of the existence of a vital principle. The latter, he argued, could not be settled by appeals to the existence of a spiritual realm; if the reality of the vital principle were capable of proof, evidence must be drawn from an examination of 'matter and its attributes'.<sup>55</sup> In this, Prichard both echoed Lawrence and prefigured a recurrent motif in the technical physiological works of the 1830s and 1840s: it was possible to talk of life without saying **anything** about God or the soul.

Prichard held that Hunter's hypothesis of a vital substance, regardless of its theological value, should be rejected because its existence could not be established empirically and because 'it is found quite inadequate to account for the phenomena of which an explanation is sought'.<sup>56</sup> The concept of a 'single intelligent principle in each organic being' or of a 'universal plastic principle, a second agent under the superintendence of the Deity', were alike useless as practical working hypotheses. In contrast, Bichat's view of life as a stream of actions linked to definite structures directed attention to a viable field of histological study. The potential of living beings to exhibit vital phenomena, Prichard concluded, should be conceived to reside 'alone in organization, meaning thereby, the union of a peculiar mechanical structure of the bodies, with a certain chemical composition of their parts'.<sup>57</sup>

It was not necessary to abolish the term 'vital principle' from physiology. Such a notion as Blumenbach's **nisus formativus** was unexceptionable, 'if understood only to designate and generalize a series of facts connected together .....; but it must not be looked upon as affording any explanation of the efficient powers by which the phenomena observed are brought about'.<sup>58</sup> True physiological 'explanation' could only consist in an elucidation of the particular textures that subserved a function.

Others expressed similar views to Prichard's somewhat more circumspectly. William Pulteney Alison, Professor of the Institutes of Medicine at Edinburgh, defended the concept of the vital principle



against its critics; but his version of what was connoted by it was much narrower than Barclay's.

Such terms as 'vitality' and 'vital power' were, Alison held, 'the general expression for those changes occurring in living bodies, which we judge to be peculiar to them'. He stressed that this 'notion of Vitality ..... has no connection whatever with the notion of Mind, as distinguished from Matter'.<sup>59</sup>

Alison drew back, however, from an explicit acceptance of the alternative hypothesis that 'the form which organized matter assumes, determines it to exhibit any particular phenomena'. Nonetheless he maintained that, because the nature of the vital principle was inscrutable, physiological enquiry could only be concerned with visible structures; its object was 'only to ascertain the conditions under which the various phenomena of life take place, and to describe and refer to general laws these phenomena themselves'. Physiological laws in this sense were, like Blumenbach's 'laws of growth', merely descriptions of organic appearances; among these organisation had a special claim to be regarded as a 'condition' of vitality.<sup>60</sup>

The principle that the structure rather than the substance of living beings was of more relevance to the explication of their properties was clearly enunciated by Cuvier at the beginning of the nineteenth century. In order to 'obtain a just idea of the essence of life', it was necessary to look to the simplest organisms; thereupon,

we readily perceive that it consists in the faculty which certain corporeal combinations have, of enduring for a time, under a determinate form, by incessantly attracting into their composition a part of surrounding substances, and rendering to the elements portions of their own proper substance.

Life, then, is a vortex (tourbillon), more or less rapid, more or less complicated, the direction of which is constant, and which always carries along molecules of the same kind, but into which individual molecules are continually entering, and from which they are constantly departing; so that the **form** of a living body is more essential than its **matter**.<sup>61</sup>

This dynamic notion of organisation tended, however, to be superseded by the more static concept of structure favoured by Bichat and his followers. This maintained that the body was a collection of tissues, each with its peculiar texture, each with its own properties; the dependence of function upon these structures was evinced by the pathological consequences of their deformation.<sup>62</sup>

It was to this model of vitality, as has been seen, that Lawrence resorted when he looked for a means of linking physiological theory closely with medical practice. Bichat's view of life as fabric promised the creation of a symbiosis in Britain between physiology and medicine comparable to that which obtained in the great Parisian schools.

Despite the risks that attended such doctrines, they began to attract an increasing number of British adherents during the 1830s. Edinburgh was the main centre for their dissemination. John Fletcher, who lectured there on physiology and medical jurisprudence during this decade, was one of the first exponents of the 'French' philosophy. Although he deferred to the sensibilities of the natural theologian by admitting the possibility of using physiological evidence to show the attributes of God, Fletcher followed Prichard and Alison in denying the connection between the existence of the vital principle and that of the soul.<sup>63</sup>

Fletcher considered the theory that life was 'an Entity, whether material or immaterial', which entered and controlled the body only to reject it. Instead, he argued that 'life' referred to 'the chain of peculiar actions ..... described as characteristic of organized beings, the immediate conditions of which are to be sought for, partially at least, in a necessary result of organism'. It followed that 'every thing relating to their Organism and Structure has a direct relation to their Life, or the Actions which they are to perform.' The study of those functions should therefore proceed through a study of living structures.<sup>64</sup>

The most important figure in institutionalising in Edinburgh this concept of life was Robert Knox (1791 - 1862), who took over from Barclay as head of the anatomy school in 1826. After an initial training in medicine in Edinburgh, Knox had travelled to Paris in 1821; there he became acquainted with the ideas of both Cuvier and of his rival Geoffroy St. Hilaire. He took something from both sources, but, in physiology, Knox's greater debt was to Cuvier.<sup>65</sup> Moreover, Knox was to become one of the means by which Bichat's style of physiology acquired wide currency in Britain.

Although he discounted the iatro-mechanism of Boerhaave and Haller's theory of elementary fibres, Knox stressed the importance of organisation in his teaching. He was, in effect, 'a ..... pioneer and exponent of what was then considered "French anatomy," as deduced from the labours of Bichat down to the last authority Béclard'. In 1826, Knox translated Béclard's **General Anatomy**

into English thereby making Bichat's schema more widely available to the British student. He boasted, with a little exaggeration, that he had 'introduced Bichat's teaching of anatomy into Britain'.<sup>66</sup>

Knox's chief importance, however, was as a teacher who assembled around him a school of students whom he steered toward a structural approach to physiology. Among these were J.Y. Simpson, John Reid, Martin Barry and W.B. Carpenter; but Knox's most outstanding protégé was John Goodsir.<sup>67</sup> Some of these remained in Edinburgh where they established a thriving centre of histological research in the 1840s. Others, such as Carpenter, went to London where they strengthened the tendency towards microscopic research fostered at King's College by Richard Todd and William Bowman.<sup>68</sup>

Carpenter is an especially interesting member of the group for two reasons. Firstly, Carpenter was the author of some of the early programmatic statements of the school in which its assumptions were explained and its position defined in relation to other physiological theories. Secondly, while not an important researcher himself, Carpenter acted as the channel through which the work of the school was made available to a wider audience. He was an industrious plagiarist who transferred the results of front-line workers from the Proceedings of the London and Edinburgh Royal Societies, and from the other learned journals in which they appeared, to a series of popular textbooks aimed at medical students. When he came to issue a new edition of any of these works, Carpenter incorporated the latest researches; in consequence, his books provide an indication of the tendency of histological inquiry in Britain in the 1840s and 1850s.

In a paper written while he was still in Edinburgh, Carpenter set out the 'first principles' of a properly conceived physiology. He adopted a basically nominalist epistemology: to say that life was a property of matter was, he claimed, merely to say that vital phenomena appeared only under certain conditions. The material pre-conditions of a given phenomenon were said to have the 'power' of manifesting that property; but it was a mistake to hypostatise this concept and to assume that, because a phenomenon occurred, 'something' must cause it. Of the 'abstract nature' of any power, Carpenter wrote, 'we know nothing; it is only recognisable by its effects; but it is ..... to be ultimately referred to the **properties** of matter, which may be regarded as axioms or postulates in any course of reasoning'. The concept of 'cause', he alleged, involved only the 'invariable connection' of certain conditions 'with subsequent phenomena'.<sup>69</sup>

In short, Carpenter sided with Hume and Mill rather than Reid on the question of causation. The occurrence of an event did not imply an agent who had performed it. Rather, consequents were to be referred to their invariable precedents: the latter could be called the 'cause' of the effect, or the 'power' of producing the effect could be ascribed to them. However, these terms did not imply the existence of any substance or entity relevant to the process other than the peculiar collocation of physical elements from which certain events followed.

When applied to physiology this theory of causation efficiently disposed of the view that the existence of vital powers showed that there was a 'distinct **intelligent agent**' at work in or on the organism. Vitality was, instead, to be referred to 'the peculiar material conditions of the tissues which exhibit them'.<sup>70</sup> Each tissue possessed its particular vital property, as well as those which were common to all, which was brought into action by the application of some stimulus. These stimuli were physical agents like heat and light, just as the tissues were material structures formed of inorganic substances.<sup>71</sup>

The path which explanation in physiology should take was, on this view, clear. An analysis of the functions of living beings must terminate 'in referring them all to certain properties possessed by their component structures'; these structures 'must, for the present at least, be regarded as an ultimate fact in physiology'.<sup>72</sup> God, in as much as he was allowed any place in this scheme, was relegated to the role of the first mover and designer of the universe who took no part in its subsequent working. It was, Carpenter held, 'degrading to the dignity of Infinite Wisdom to suppose that, at the creation of each world, he found it necessary to delegate to a subordinate the control over its workings, instead of at once impressing upon its elements those simple properties, from whose mutual actions' all future contingencies would proceed as God had foreseen them.<sup>73</sup> More to the point, the supposition of a deity acting on the organism either directly or through intermediaries was a distracting irrelevance to the form of physiological reasoning that Carpenter was recommending.

The fact that a contest between competing conceptions of physiological explanation was taking place was demonstrated more clearly in the article on 'Life' which Carpenter contributed in 1847 to Todd's *Cyclopaedia of Anatomy and Physiology*. There he argued that for the 'old philosophers' it 'was considered as a sufficient explanation of any phenomenon to apply to it some abstract term, expressing a vague idea of a property inherent in the body which exhibited it, without attempting to

ascertain the conditions of its operation.' Such philosophy tended to 'regard all matter, at least the grosser forms of it, as essentially inert, and therefore to attribute all spontaneous motion to a union of the thing moved with some substantial moving cause'.<sup>74</sup>

But this was to infect physiology with theological concerns and to confuse vitality with spirit: on these assumptions, 'to set aside the doctrine of a vital principle, — necessarily implies the relinquishment of the idea of mind as a distinct existence. 'For this reason, Carpenter rejected the use of 'vital force' made by Abernethy, Prout and Johannes Müller. In place of their denudation of matter of all power, it was necessary to recognise that all matter had a **potential** for vitality under certain circumstances: the most important of these conditions being organisation.<sup>75</sup>

Such programmatic utterances left much undefined, however. They did nothing to specify **what** level of organisation was most relevant to function. In fact, such questions were not answered by proclamation but by the combination of technical and conceptual resources available to physiologists. Between 1840 and 1850 these resources underwent a drastic change; in consequence, the locus of active organisation in the body shifted. In part, this change reflected the waning authority of French models of vitality and the growing importance of German physiological anatomy for British workers seeking some exemplary piece of research. Carpenter's textbooks represent the filtered residue of these processes and thus illustrate the changing focus of histological attention during this decade.

These events can be illustrated most clearly by the treatment in Carpenter's works of a particular function, secretion, and its organ, the gland. In 1839, he defined the gland as 'a bag or sac, formed of a membrane on the outside of which blood vessels ramify, and provided by an orifice by which the contents may be either transmitted to the place where their presence is required, or carried out of the system altogether'. He continued that it was the **membrane** of the sac which was the true secreting organ. Since function followed structure, there were, presumably, differences in the form of the membranes of different glands which determined the nature of their secretion. But Carpenter confessed that 'our means of observation do not at present enable us to distinguish any marked difference in its [the membrane's] structure in different glands'.<sup>76</sup>

In the 1842 edition of his **Principles of Human Physiology** Carpenter similarly wrote of secretion that

Of the reasons why certain compounds forming parts of the circulating blood, are separated from it by one organ, and others by a different one, nothing whatever is known; and there is nothing in the evident structure of these organs, that can afford any clue to the attainment of such knowledge. When their ultimate structure is considered, it is found to be neither more nor less than a vascular membrane, made up into various forms for convenience of packing.<sup>77</sup>

But by 1846 this passage had been significantly amended:

Of the reasons why certain compounds forming parts of the circulating Blood, are separated from it by one organ, and others by a different organ, no other account can be given than that which refers them to the special endowments of the cells, which are the real instruments of the process. When the ultimate structure of the Glands is considered, it is found to be neither more nor less than a vascular membrane, covered with epithelium-cells, and made up into various forms of packing.<sup>78</sup>

This abrupt resolution upon the cell as the physiologically crucial structure was most evident in Carpenter's treatment of particular organs — for instance, the liver. In 1842 he held that the smallest particles called 'lobules', which were visible to the naked eye, were the key to the organisation of this gland. The 'structure of each lobule ..... gives us the essential character of the whole gland'. This structure was either pentagonal or hexagonal, penetrated by passages through which blood vessels passed, and permeated by minute yellow particles called 'acini'.<sup>79</sup> By 1846, Carpenter held that the 'liver may be regarded as essentially consisting of a mass of cells, in connection with the ramification of the Hepatic Duct.'<sup>80</sup>

In 1855, Carpenter went as far as to claim that it was the presence of bile-secreting cells which constituted the liver qua gland.<sup>81</sup> In this respect, the liver represented the general type of minute glandular structure. The membrane along which the hepatic cells were distributed was undulating; the 'pits' that were so formed Carpenter called 'follicles'. It was the cells which lined these that were the true agents of the secreting process. The follicles were, therefore, 'the simplest type or examples of all the glandular structure, by which certain products are separated from the blood.'<sup>82</sup>

The revised 1855 definition of the gland was:

a closely-packed collection of follicles, all of which open into a common channel, by which the product of the glandular action is collected and delivered. The follicles contain the secreting cells in their cavities; whilst their exterior is in contact with a network of blood-vessels, from which the cells draw the materials of their growth and development.<sup>83</sup>

Further, whereas in 1839 no structural variation had been suggested as an explanation of the different secretions produced by glands, in 1855 Carpenter held that

The peculiar power by which one organ separates from the blood the elements of the Bile, and another the elements of the Urine, whilst a third merely seems to draw-off a certain amount of its albuminous and saline constituents, is obviously the attribute of the ultimate secreting cells, which are the real agents in the secreting process.

The 'ultimate facts' of physiology were now cells.<sup>84</sup>

Within this passage towards the cell as the centre of histological attention, a more particular change is discernible. While in the mid-1840s Carpenter's descriptions of cells were cursory and imprecise, by the mid-1850s they possessed a much greater degree of morphological detail. In 1847 he was content to write that a cell was 'a minute bag or vesicle, formed of a colourless membrane, in which no structure can be detected; and having its interior filled with fluid of some kind.'<sup>85</sup> In 1859, this definition was amended to include mention of the intracellular matter called the 'nucleus'. Moreover, far more detail on the shape of particular cells appeared, in some cases with suggestions as to how these forms might affect their functional properties. Thus Carpenter described the filaments or 'cilia' on the epithelial cells and concluded that these 'are organs of great importance in the animal economy, on account of the extraordinary motor powers with which they are endowed.' The purpose of the cilia was to propel fluids over the cellular surfaces, and thereby to expedite their secretory function.<sup>86</sup>

Three major transitions are therefore evident in Carpenter's works of the 1840s and 1850s. The first was toward a recognition of the cell as the agent of physiological processes. The examples that have been given from discussions of secretion could be supplemented by others showing a similar elevation of the cell to an active role in nutrition and reproduction. Secondly, there was a tendency towards a finer definition of histological structures in general. Thirdly, there was a corresponding

increase in the specificity of cell morphology. The textbooks were, as usual, several years behind in recording these shifts, which were aspects of a fundamental transformation in biological thought and practice between 1839 and 1859.

The history of the 'cell theory' has been exhaustively explored elsewhere.<sup>87</sup> Here I am only concerned to describe its impact upon physiological anatomy in Britain, and particularly upon the theory and practice of the Edinburgh and London schools of histology. The cell theory was made possible by technical developments of the microscope early in the nineteenth century; however, these developments, far from narrowing the options available to those seeking the basic unit of life, provided such a proliferation of contradictory observations that throughout the century the nature of cell structure and agency were matters of intense and convoluted controversy.

In 1829, Prichard had noted the microscopical researches of Dutrochet, Duman and Milne Edwards in France which, according to some, had established 'new and very remarkable opinions as to the mode of subsistence of living bodies'.<sup>88</sup> These workers had claimed that the intimate structure of the body was composed of 'globules', a concept which was the indirect ancestor of the 'cell'. In 1836, R. Willis embraced this view, asserting that 'Globules ..... are to be regarded as the elementary constituents of organized bodies, as the ultimate molecules possessing a distinct form, which by their aggregation compose them.' The difference in the form of the various tissues was, he claimed, the result of the different structures of their constituent elements.<sup>89</sup>

Others were less sure. R.D. Grainger in the same volume restated the older view of the cell as a mere 'cavity' in a network of fibrous matter, rather than as an entity.<sup>90</sup> Carpenter echoed these doubts, arguing that the search for 'ultimate atoms' of life was a futile exercise.<sup>91</sup>

This confusion gradually gave way in the course of the 1840s to a consensus. In 1855 T.R. Jones was able to write: 'That all animal and vegetable tissues are primarily either composed or developed from cells is a fact now wholly recognised.'<sup>92</sup> This consensus proved, however, to be singularly fragile. What altered initially ambivalent attitudes to the cell theory was the association of the concept of the cell with a successful line of histological practice. The 'cell' was established as the framework within which the project of a physiological anatomy could proceed at a microscopic level.

This afforded an extension of technique and interpretative opportunities, without any basic change



in assumptions, to those who had received Bichat's principles from Knox and other advocates. As Goodsir's biographer pointed out the transition seemed a natural one: although Bichat's *Anatomie Generale* had 'satisfied more than one generation', the histologists who began their independent researches in the late 1830s were attracted to a new style of work and to a new set of exemplars. Prominent among these were the German microscopists, such as von Baer, Johannes Müller, and Schleiden. By the early 1840s these were complemented by a growing native tradition of similar work. In Edinburgh the two major figures were John Goodsir and Martin Barry: the latter specialised in the role of the cell in ontogeny<sup>93</sup>; but it was the former whose work was usually regarded as the keystone of the school.

Goodsir, for instance, pioneered the notion of the cell as the agent of secretion. In an address to the Medico-Chiurgical Society of Edinburgh in 1842 he laid stress upon the fibro-cellular framework of the kidney, and concluded that 'the urine is formed at first within the so-called epithelium-cells of the ducts; and ..... these burst, dissolve, and throw out their contents, and are succeeded by others which perform the same functions.' The granular 'acini' of the kidneys were, he claimed, composed of several of these nucleated cells.<sup>94</sup>

In a series of lectures given to the Royal College of Surgeons in 1842 - 3, Goodsir extended this model to different organs and functions. While other British histologists, such as Bowman in 1840 and Barry in 1841, had recognised the existence of nucleated cells in the body, they had regarded them as merely embryonic structures. In contrast, Goodsir insisted that such 'minute cellular parts' persisted in the tissues of the adult and were the true centres of vitality. Just as the entire organism originated from a single cell, so each cellular centre in the body generated the parts which composed the substratum and active element of each organ. The cell was, therefore, the genetic as well as the structural and functional base of the organism.<sup>95</sup>

Goodsir thus pre-empted Virchow's doctrine that all cells developed only from other cells. Moreover, he anticipated the latter's concept of the 'cell-territory', within which cells of a particular type reproduced themselves from a centre and acted in physiologically determinate ways. Finally, by his idea that each cell-centre was an independent agent, Goodsir suggested the 'colonial' view of the organism which was also later linked to Virchow. As Carpenter expressed this doctrine: 'in the Animal as in the Plant, each integral

portion of the Organism possesses an independent life of its own, in virtue of which it performs a series of actions peculiar to itself, provided that the conditions requisite for those actions be supplied.<sup>96</sup> The cell was therefore the necessary and sufficient condition of life. By implication, it was also the necessary and sufficient object of enquiry: on the 'single cell', Carpenter wrote, 'the physiologist bases his idea of the most elementary type of Organization; whilst its actions present him with all that is essential to the notion of life.'<sup>97</sup>

There were three main lines along which investigation into the nature of the cell could proceed: cells could be regarded as centres of secretion, nutrition or metabolism and of growth. The first and last of these lines of research were extensively developed in Edinburgh itself during the 1840s; the role of the cells in secretion was Goodsir's own specialty, while Barry concentrated upon the latter field. 'Nutrition' tended to be the province of London rather than of Edinburgh histologists: first William Bowman and then Lionel Beale made detailed studies of the development of the different tissues from cells.<sup>98</sup> It is important to note that the elucidation of how the cell subserved a particular function was largely a matter of specifying the details of cellular morphology in a given case. Attempts were occasionally made to refer this structure in a mechanistic way to the function, as in the case of the cilia mentioned above; but this was unusual. 'Explanation' was held to consist in simple description.

By the early 1850s the ubiquity of the cell in all functions had been elevated to the status of a near dogma. In Todd and Bowman's words, 'the nucleated cell is the agent of most of the organic processes, whether in the plant or the animal, from the separation of the embryo from its parent, to the development, growth, and nutrition of the adult individual'.<sup>99</sup> The doctrine was both the foundation of a viable programme of research, and it had been made a central resource in the medical pedagogy of the time. Goodsir, for example, who was from 1845 Professor of Anatomy at Edinburgh, predicated his lectures on the claim that each cell was 'a completely organized structure' which underlay all function.<sup>100</sup>

However, the centre of research activity had by this time shifted to the south. In King's College London the techniques and concepts which constituted the programme of minute physiological anatomy were by 1855 developed to their most elaborate state. Lionel Beale succeeded Bowman as Professor of Physiology and General and Morbid Anatomy in 1853. He was probably the most accomplished of the

British microscopists of the period, and in his papers and lectures he stressed the dependence of the 'structures' which his school had posited as the basis of life upon a particular manipulation of the materials and instruments available.

Thus Beale began his 1856 discussion of the minute anatomy of the liver with an account of the chemical treatments needed to harden the tissue before it was possible to make sufficiently thin sections. Such sections were transparent and their structure consequently difficult to 'see'. It was therefore necessary to stain them or to inject them with a dye which would 'reveal' their inner organisation. For example, Beale admitted that in order to show the continuity of the cell-containing network of the liver with the ducts, he had to inject the latter with an ink.<sup>101</sup>

By using such methods and different powers of magnification Beale proceeded to a progressively more minute account of the structure of the liver. He defined the liver 'lobule' as a 'solid network of capillary vessels in the meshes of which the liver-cells are seen'. But they were only 'seen' when certain provisions had been made: 'In a properly prepared liver it is often possible to demonstrate the cell-containing network in one section, and the capillary network in another.'<sup>102</sup> Similarly, through the injection of dyes it was possible to show that the cells formed lines radiating from the centre of each lobule.

These cells were the 'most important anatomical elements' of the liver. Beale described their form in detail: all were nucleated, though the shape of the nucleii varied; sometimes a 'bright spot' or nucleolus could be discerned on the nucleus. Beale was uncertain, however, about the existence of a 'cell wall' as such; he acknowledged that the abolition of this structure went against the weight of histological opinion, but preferred to take an agnostic position himself.<sup>103</sup> He had no doubt, however, that it was the cellular system, together with the capillary network, which were the means whereby the liver performed its functions, and in this it mirrored the essentials of all glands.<sup>104</sup>

When, through his lectures at King's College, Beale tried to perpetuate this vision of the organism, he was still more explicit about its reliance upon 'proper' technique. Success in the field of microscopic research was, he held, 'in great measure dependent upon our knowledge of the various methods which experience has shown to be advantageous for rendering the anatomical peculiarities of a texture clear and distinct'. So, while the topic of microscopic manipulation might seem dry, 'these are questions not beneath the consideration of any one who takes a real interest in the structure of the different organisms by

which he is surrounded'.<sup>105</sup>

It was not enough to 'place an object in the field of a microscope, in order to make out its structure'.<sup>106</sup> An elaborate preparation of the tissue was necessary; to distinguish each level of organization a different chemical treatment and microscopic strategy was required. For example, insoluble saline materials often 'prevent us from seeing the anatomical elements of which a tissue is composed': the addition of dilute acid removed this barrier to perception. Similarly, the action of acids and alkalis rendered opaque transparent 'structures'. The cell wall might be too opaque 'to enable us to see the nucleus in the interior of the cell'; but treatment with a suitable reagent would make the membrane 'perfectly transparent with the nucleus well defined in its interior'.<sup>107</sup>

Beale acknowledged that not all histologists admitted the validity of these 'methods of preparation'. They had objected that, far from revealing the true nature of tissue, 'by these processes, structures are even formed which have no real existence in the natural state of the part'.<sup>108</sup> Beale would not concede this. He insisted that the structures that he and his fellow-workers dealt in were 'real'. He did, however, allow that their reality was not obvious; the student had to learn to see what was there: 'The eye of the observer requires much careful education before he is able to appreciate fully the character of the structure he is examining'.<sup>109</sup>

Beale therefore urged caution upon his students. If they could not, at first, recognise any of the structures an author had described, they should not 'too hastily conclude that the author has been recording the results of his imagination, rather than observed facts'. Instead of relying upon his own observations the neophyte in microscopical science should defer to the authority of the school – to the received model of organisation into which he should make his own results fit.<sup>110</sup>

In effect, the structures with which Beale dealt were the artefacts of the methods and concepts of his school: they were **manufactured** by the techniques and preconceptions brought to the microscope. The interest which underlay the procedures of these physiological anatomists was to discern clear, definite structures, whose morphology formed the basis of their accounts of vitality. These structures might be regarded as stable or as stages in a continuous process. In either case, discrete, determinate forms were needed because cellular change was described, not in terms of a gradual transition, but of

a series of 'stills', each of which represented a distinct mode of organisation.<sup>111</sup>

This approach to the organism had, in the context of mid-nineteenth century Britain, many advantages. It was easily integrated with the anatomical concerns of the medical profession, for instance, and could be undertaken by workers who drew their income from teaching trainee physicians and surgeons. Above all, however, this style of research was suited to the state of life science in Britain at this time. Microscopy was atomistic: it could be undertaken by individual researchers with little assistance from others. And it was cheap: the outlay on equipment was relatively small. Given the lack of an extensive institutional basis for the discipline, and the poverty of British science in general, physiological anatomy was an accommodation to the reality in which physiology had to operate during these years.

To some, however, this accommodation appeared to be more a capitulation. Instead of accepting the restrictions upon physiological research inherent in the present state of affairs, they strove to overcome them. They enlarged their research ambitions and demanded the means with which to realise them. They also rejected the model of the organism with which their predecessors had worked, and opted for one more conducive to this strategy.

### iii. The Stuff of Life

It was in London that a rival to the structuralist theory of life was developed in the 1850s. While King's College was the seat of the structuralists, their rivals worked from the Royal School of Mines, later from its successor the Normal School of Science at South Kensington, and from University College. The earliest and most articulate proponent of the view that life should be regarded as a function of substance, not of form, was T.H. Huxley.

Huxley first adumbrated this argument in an 1853 address at the Royal Institution. He proposed to discuss 'organisation' as the character which distinguished the living from the non-living; however, Huxley made it clear that his notion of the characteristic structure of organisms was quite different from that of, say, Goodsir. Huxley viewed tissue as a 'cellular matrix' which, 'though at first unquestionably a homogeneous continuous substance, readily breaks up into definite portions'. Each of these portions, or 'endoplasts' was surrounded by a less completely differentiated section of the matrix; these Huxley called 'periplasts'. He noted that certain writers had considered the endoplast and periplast together to constitute a separate entity called a 'cell', and had referred to the inner segment as the 'nucleus' and the outer as the 'cell wall'. Huxley held this view to be wrong and tried to show that

the existence of separate cells is purely imaginary, and that the possibility of breaking up the tissue of a plant into such bodies, depends simply upon the mode in which certain chemical and physical differences have arisen in a primarily homogeneous matrix.<sup>112</sup>

Two ends were served by this formulation. Firstly, attention was diverted from the cell — *qua* discrete structure — as the basis of life. Huxley justified this move by claiming that explanation of function in terms of structure had led to anti-naturalistic conceptions. In fact, the emphasis upon the cell had been the means whereby 'vital powers' had reasserted themselves. The notion of the cell as a centre of power had led to the hypothesis of a 'cell-force', akin to the old-style vital principles. Further, the idea of the cell as structure had implied the existence of a formative influence which created and maintained the edifice. This mode of reasoning, Huxley argued, was 'unphilosophical'; he made his point by way of an analogy with human constructions. While it was true that bricks and mortar were components of a house, no-one would think that 'the house was built by brick-force ..... But this is

just what has been done with the human body – We have broken it up into “cells”, and now we account for its genesis by cell-force.’<sup>113</sup>

Huxley did not specify the ‘authors’ to whom he was alluding, but the cap fits Virchow and Goodsir. The former had placed great stress upon the importance to physiology of the notion of ‘cell-force’, though the exact ontological position of this entity in his system remained unclear.<sup>114</sup> Goodsir had gone to extremes in abstracting the form from the substance of the body; he began to speculate in an almost neo-Platonic manner upon the immanence of geometric constants within the organism. He held, for instance, the logarithmic spiral to be ‘a teleological chart in nature’s beautiful designs.’ From the existence of such design Goodsir inferred the action of a ‘vital force’, which operated according to fixed patterns.<sup>115</sup>

One answer to such vitalism was an attack upon the primacy of form in physiological explanation. In particular, the cell, through which the vital force acted, had to be denied the status of a real entity. To do this, Huxley reverted to an older, Cuvierian, view of the cell as a cavity or discontinuity in a homogeneous substance.

The second goal of Huxley’s argument was to identify the real causes of vitality as immanent in this matrix. He held that the changes in the body were of two kinds: chemical and morphological, and that the latter were dependent upon the former. The ‘cell’ or endoplast played no part in morphogenesis; on the other hand, the periplast, or undifferentiated substance, acquired, by ‘chemical change or deposit ..... Horn, Collagen, Chondrin, Syntonin, Fats, Calcareous Salts, according to whether it becomes Epithelium, Connective Tissue, Cartilage, Muscle, Nerve or Bone.’<sup>116</sup> At a minute level, these changes often produced a difference between the substance in the neighbourhood of the endoplast and that beyond it; the result of this accidental discontinuity had been the false notion of distinct histological elements as *agents* of these changes. In fact, chemical differentiation was always the true first mover.

Huxley elaborated his argument in an article on ‘The Cell Theory’ in the same year. This document became, effectively, the manifesto of the ‘progressive’ party in British physiology in the next two decades; the major planks of the platform of this movement corresponded to the polemical goals of the article. The first was to establish a naturalistic idiom of explanation for physiology

from which vital forces, and all they implied, were excluded. According to Huxley's biographer, 'Physiological study in England at this time was dominated by transcendental notions'. This was an exaggeration; however, as Goodsir's and later Beale's espousals of vitalism showed, the intrusion of theological concerns into physiology continued into the 1860s and beyond. Such a confusion about what the aims and standards of the subject should be militated against its establishment as a distinct field of activity: 'transcendental notions' were, in effect, inimical to professionalism. Huxley attempted to overcome this obstacle by the epistemological tools that were also used in other aspects of the naturalist strategy. In Michael Foster's words, Huxley's paper 'The Cell Theory' drove 'the sword of rational inquiry through the heart of conceptions, metaphysical and transcendental, but dominant'.<sup>117</sup>

At the same time, the members of the progressive party in physiology sought to found a certain kind of research programme for their science; one which would, it was supposed, conduce to professionalism in another way. As Leonard Huxley put it, around 1853, 'To put first principles on a sound experimental basis was the aim of the new leaders of scientific thought'.<sup>118</sup> Huxley's model of vitality, with its notion that life was the result of chemical, and to a lesser extent, of physical change provided a totem for this project. The study of such causes, it was alleged, could only take place in the laboratory.

Huxley attempted to guarantee a naturalistic form of explanation in physiology by a 'positivistic' philosophy. In this he was, to a considerable extent, merely following Carpenter. Whatever material conditions were identified as precedent to a given vital phenomenon, it always remained open to ask what 'caused' those conditions to have that effect. Thus, if life were defined as the result of molecular forces, it would 'doubtless be said by many, But what guides these molecular forces?'. Some 'force' would be invoked be it called 'Archaeus', 'Bildungstrieb', or 'Vital Force'.<sup>119</sup>

Unlike some of his later statements on this subject, Huxley did not dismiss such enquiries after 'efficient' causes as in all cases misguided. But he did assert that they were inappropriate to natural science, and especially to physiology. There were, he argued, two separate spheres of knowledge: 'Physiology and Ontology' which could not 'be too carefully kept apart'. There might be such



'causes, powers and forces' as the vitalist attributed to the organism,

but they are the subject of the latter, and not of the former science, in which their assumption has hitherto been a mere gaudy cloak for ignorance. For us, physiology is but a branch of the humble philosophy of facts; and when it has ascertained the phenomena presented by living beings and their order, its powers are exhausted. If cause, power, and force mean anything but convenient names for a mode of association of facts, physiology is powerless to reach them.<sup>120</sup>

A physiology which was in this way confined to observation of phenomena and to ordering them with nominal concepts could not become entangled in metaphysical and theological issues. However, what counted as the relevant 'phenomena presented by living beings' was another question. Huxley spent much of the paper arguing that the phenomena of 'organisation', as the term was usually understood, were **not** of primary physiological interest.

Huxley did not doubt the value of anatomy and histology as a preparation for physiology; but he did deny that an 'anatomical' explanation, however minute the elements of organisation with which it dealt, was appropriate to the physiologist. The method of relating function to structure, whose origin was usually attributed to Bichat was, Huxley argued, of much older provenance. It could be found in the sixteenth century *De Partibus Similaribus* of Fallopius. 'Cells' had taken the place of Fallopius's 'particles', and the powers which worked through these structures were no longer called 'animal spirits' but 'vital' or 'cell' forces. Otherwise, Huxley alleged, 'this passage would serve very well as a concise summary of the "cell-theory," such as may be found in many a hand-book of the day. So far, and no further, have three centuries brought us'.<sup>121</sup>

To escape from this dead end of enquiry Huxley argued that it was necessary to go beyond the cell and, indeed, to abandon the goal of a structural explanation altogether. The exemplar he chose to illustrate an alternative mode of argument was the eighteenth century German embryologist Caspar Friedrich Wolff. Wolff had challenged the explanation of ontogeny offered by Bonnet and Haller, which assumed the existence of the pre-formed parts of the embryo in the body of the parent. Instead, Wolff held that growth proceeded from a structureless seminal fluid whose **substance** must therefore contain the germs of life. In contrast to the concept of matter as simple and

inert favoured by the preformationists, Wolff saw it as possessed of 'qualities, modes, and attributes'.<sup>122</sup> The differences in the growth of different organisms, and between the parts of the same organism, were not to be referred initially to peculiar forms of organisation, but to the different qualities of the vital fluid.

Huxley extended these notions to physiology more generally. In particular, Wolff's ideas could be used to rectify the widely-accepted fallacies inherent in Schleiden and Schwann's conception of the cell. Up to a point, all three held compatible views: they agreed on the existence of a structureless matrix from which forms evolved. But thereafter Schleiden and Schwann diverged from Wolff in their claim that 'the primary histological element (cells) are independent, anatomically and physiologically', and that they were the centres of causal power in the organism. Wolff, on the other hand, correctly maintained that 'the primary histological elements ..... are not either anatomically or physiologically independent; that they stand in the relation of **effects** to the organizing or vital force'.<sup>123</sup>

The true cause of life was, therefore, prior to even the simplest organisation. 'Cells' were 'not instruments, but indications ..... they are no more producers of vital phenomena than the shells scattered in orderly lines along the sea-beach are the instruments by which the gravitational force of the moon acts upon the ocean.'<sup>124</sup> This was a position to which many vitalists would subscribe. Huxley, however, did not propose to look beyond matter for the power which generated life; he wished only to take the search for the physical basis of life one step further back — into the molecular constitution of matter. The faculty of manifesting vital properties 'resides in the matter of which living bodies are composed, as such — or, to use the language of the day, ..... the "vital forces" are molecular forces'.<sup>125</sup>

Huxley elaborated on this notion in his 1858 lectures at the Royal Institution on 'The Principles of Biology'. Discussing the yeast plant as one of the simplest examples of the essentials of life, he stressed its chemical composition, especially its proteinous character, and the cyclical changes through which this substance passed. In contrast, form was only mentioned to point out the absence of structure in the plant. In its dependence upon a peculiar chemical composition for its functions, and in the independence of these processes of organisation, the yeast plant, Huxley argued, was typical. Such, in the most elementary terms, was a 'Living Being', and, 'Life or Vitality consequently is a name for the

sum of the changes undergone by Protein matter.'<sup>126</sup>

These changes could be classified under the headings of 'Absorption, Metamorphosis, and Irritability'. In the simplest organism all these were already evident; however they took place in the 'Total absence of organisation' and in the 'Total absence of Histological differentiation into cells or nuclei'. It was only in the higher animals that functions were distributed among special parts; such organisation was, therefore, a secondary feature of life.<sup>127</sup> On the other hand, the protein substance was ubiquitous: 'Life in Man as in the Amoeba depends on the absorption of Protein — and the Metamorphosis of this into the substance of his tissues'.<sup>128</sup>

This theoretical statement could be translated into methodological terms. Just as the notion of life as the property of a fabric justified a morphological style of enquiry, so the identification of life as a function of substance commended a chemical analysis. In essence, physiology was the study of body chemistry, of the transformations of the compound called protein, to be conducted with the concepts and tools of the inorganic chemist. The enunciation of such a programme had a dual significance in the history of British biology.

It was, in the first place, indicative of a growing specialisation in the life sciences between 1850 and 1870. In the course of that period, 'physiology' ceased to be an omnibus term for all kinds of biological enquiry, and gradually acquired an identity and province of its own. Although its relations with anatomy and histology, both conceptual and institutional continued, it was held that physiology was capable of being studied with its own techniques and assumptions and in its own setting. At the same time, the other branches of biology also redrew their boundaries and established research goals peculiar to themselves.

By 1877 it was possible to classify the departments of biology, and to claim that their efforts were complementary and did not overlap: each regarded the organism **in its own way**. While morphology regarded the body as a texture, possessing an external form and an inner structure, the subject of physiology was 'the operation of certain forces, in virtue of which [the organism] undergoes internal changes, modifies external objects, and is modified by them.' In short, for the physiologist, the body was 'a molecular machine of great complexity', whose chemical and physical workings produced the sum of vital phenomena.<sup>129</sup>

Further, the 'substantial' or, as it came to be known in the 1860s, the 'protoplasm' theory of life represented an attempt to secure a certain type of social organisation for physiology in Britain. As previously remarked, the histological school's research programme was a concession to the fragmentary nature and lack of resources of mid-Victorian life science. The practitioners of that school had, moreover, a vested interest in upholding the value of their techniques and in belittling rival approaches. In particular, they were indifferent or even hostile to laboratory investigations. These were rejected as 'artificial', especially when live animals were the objects of experiment; as Todd and Bowman put it:

Doubtless, many obscure points have been elucidated by experiments on living animals, and discoveries have been made which have greatly contributed to the progress of physiology; but the best physiologists are ever reluctant to interrogate nature in this way, knowing that replies elicited by torture are rarely to be depended on.<sup>130</sup>

In contrast, after the mid-1850s, a new party, centred on London, began to advocate a basic change in both the institutional and in the methodological bases of physiology. Many of these men had received a training on the Continent. There were enough British physiology students in Paris in the 1850s to form their own society: among them were J.S. Burdon Sanderson, F.W. Pavey, and W. Rutherford.<sup>131</sup> Still more important, however, was the German connection. Some of the most prominent British physiologists of the later nineteenth century studied at German universities, including E.A. Schaffer, W.H. Gaskell, V. Horsley, and E. Ray Lankester.<sup>132</sup> Others, like Michael Foster, spent time in the German physiology schools without undertaking any formal instruction there.

While a previous generation had looked to the example of Bichat and Virchow for a model of how to do physiology, these workers took the tradition of Magendie and Bernard and of the German physicalists as the exemplar on which they tried to found <sup>their</sup> own practice. In Burdon Sanderson's words: 'we accord to Müller and to his successors Brücke, du Bois-Reymond, Helmholtz, who were his pupils, and Ludwig in Germany and to Claude Bernard in France, the titles of founders of our science.'<sup>133</sup> When it came to naming specific 'exemplary' pieces of works that had been derived from this tradition, Burdon Sanderson listed J.R. Mayer's demonstration in 1845 that the principle of the conservation of

energy applied to organic processes; Bernard's work on the **chemical** mechanism whereby this energy was stored and released; Helmholtz's success in measuring the passage of the impulses of the nervous system; du Bois Reymond's investigations into the electrical phenomena of organisms; and Ludwig's experiments on the hydraulic principles of circulation. Such efforts, he concluded, had produced 'a complete revolution in the ways of thinking and speaking about the phenomena of life.'<sup>134</sup>

Two things need to be noted about these exemplars. First, they all, in different ways, assumed a mechanistic theory of life; second, all were the result of experimental studies. Theory and practice were linked, inasmuch as the presumption that vital phenomena were merely special instances of the working physical and chemical forces sustained the claim that physiology must be carried on by methods that could only be deployed in a particular setting, the laboratory, which had become the characteristic matrix of Continental physiology. This was to go against established British assumptions and practice, which were predominantly histological and individualistic. As early as 1854, Huxley challenged these prejudices: 'Physiology not an experimental science? ..... It would be much more true to say that Physiology is **the** experimental science **par excellence** of all sciences; that in which there is least to be learnt by mere observation, and that which affords the greatest field of exercise of those faculties which exercise the experimental philosopher.'<sup>135</sup> He illustrated the point by reference to Bernard's chemical researches upon the functions of the liver.

There were great difficulties in the way of the emulation of such procedures by British physiologists. The so-called stagnancy of British physiology between 1840 and 1870 was, in fact, the evidence of a growing gap in the **means** with which physiology was pursued in Britain and on the Continent. The 'backwardness' of physiology in Britain was a direct result of the relative paucity of resources for science there compared with France and Germany: the discrepancy was epitomised in the contrast between the solitary British microscopist and the teams of workers employed in the laboratories of the German universities. What permitted the growth of Continental physiology and what prevented the British from following in the same path 'was the whole interrelated pattern of the professionalization and the state of the universities in each country.'<sup>136</sup>

There were two related imperatives for those who would establish an experimental physiology on the Continental model in Britain. The first was to propagate a model of vitality which directed

attention away from purely anatomical and histological concerns toward a dynamic view of the body as the site of a series of physical and chemical changes. The other was to obtain sufficient resources to enable these phenomena to be studied with the same methods and institutions current in France and Germany. Thereby, the employment opportunities necessary for the establishment of a true physiological profession in Britain would be realised.

The substantialist or protoplasmic theory of life served the former interest, and, indirectly therefore, the latter. The connection between this conception of the organism and an experimental, particularly a chemical, methodology was not confined to Britain. In the 1860s German workers challenged the conception of the cell put forward by Schleiden and Schwann; in place of the notion that 'Form and visible organisation were ..... necessary for the manifestation of life', they transferred the power of displaying vital phenomena to 'utterly indefinite and formless masses of protoplasm'.<sup>137</sup> In effect, they endorsed Huxley's utterances of the 1850s, though in a different idiom.

The most vocal German theorist of the *Urschleim* was Ernst Haeckel. His concern was to construct a cosmology in which material nature appeared as an autonomous, uniform entity.<sup>138</sup> If nature were indeed unitary, then no basic discontinuity should appear at the point at which the non-living passed into the living. Haeckel therefore conceived the rudimentary form of life to be an unorganised precipitate of inorganic matter, scarcely distinguishable from the substance from which it had emerged. These primitive life forms Haeckel called 'monera'.

Huxley collaborated with Haeckel and expressed similar ideas on the origin of life. For example, Huxley 'discovered' the empirical verification of Haeckel's speculative monera. In 1868, after examining mud dredged from the bottom of the Atlantic, Huxley told the BAAS that he had found lumps of a transparent gelatinous structureless matrix. He argued that this substance was alive: it was a sub-cellular organism, which Huxley named *Bathybius haeckelii* in honour of the man who had predicted its existence. In fact, Haeckel had created an empty category within his system which Huxley endeavoured to fill. He did so because monera supplied a link between the organic and the inorganic and so endorsed a physicalist theory of life which detached vitality from organisation.<sup>139</sup>

More important in the professional context than Haeckel's theories were the writings of German cytologists like Max Schultze, Ernst Brücke, and Willy Kühne in the 1860s. These gradually eroded

themorphological characteristics which had been used in the previous decade to define the cell. First the membrane and then the nucleus were eliminated as **necessary** conditions of cellular action. The cell itself, eventually, was in danger of disappearing as a physiologically significant concept. In its place was left bare protoplasm, and the practice of these 'cytologists' consisted in speculating 'upon the nature of protoplasm and ..... celebrating its amazing properties'.<sup>140</sup>

Similar doctrines had two centres in Britain, both in London. The first was the circle around Huxley at Jermyn Street and then at South Kensington; the other was at University College. Each centre pursued an aspect of the strategy of professionalisation; in particular, both attempted to gain a place for physiology at all levels of the educational system. There was, however, some division of labour between them.

Huxley was especially concerned to secure physiological teaching in the schools. To this end, after 1871 he undertook an annual course of instruction for school-masters in which he stressed the importance of 'practical work in the laboratory' even in an elementary physiological education.<sup>141</sup> By 1872, Huxley had succeeded in extending this course to full-time students. The course comprised an essentially mechanistic concept of life: the body, Huxley maintained, 'constantly exerts **mechanical force**, gives off **heat**, evolves **carbonic acid** and **water**, and undergoes a loss of **substance**'.<sup>142</sup> In discussing the location of the processes, Huxley discounted the importance of the 'formed' parts of the tissue and stressed the role of unformed substance. Vitality was a matter of physical and chemical process rather than of structure.<sup>143</sup>

Huxley attached great importance for the future of life science in Britain to this course. He regarded the teachers whom he trained as 'scientific missionaries to convert the Christian Heathen of these isles to the true faith'.<sup>144</sup> The elementary physiology teaching which, he hoped, they would eventually perform would be multi-functional. Through such instruction, teaching opportunities for physiologists would be increased; the number of students with the basic knowledge to progress to advanced physiology would be augmented, and, through the creation of a body of science teachers in the secondary schools, the influence of scientific culture would be spread more widely in an area where it might encourage future demand for scientific education at the university level.

A complementary strategy occurred in the universities themselves. There the main interest was

to create a body of physiologic researchers and teachers servicing a guaranteed student population. At University College London, in particular, efforts were made both to create new facilities for physiology and to reroute existing courses onto experimental lines. This, in large part, was a matter of developing a new form of pedagogy which involved extensive use of chemical and physical conceptions of function and, with them, of experimental methods. In keeping with the competences of most of the lecturers, 'physiological' instruction in the medical schools had previously been almost exclusively anatomical and histological in character. In the 1860s, the first cautious steps away from this state of affairs were taken by Michael Foster at University College, where a new style of physiology course was pioneered. Foster did not at first eschew the use of histological methods in physiological education: the fixed capital invested in microscopes and the other paraphernalia of this form of study was too great to be abandoned immediately. But he did begin to qualify the exclusive relevance of histology to the physiologist and to suggest that other forms of teaching were more relevant. As he wrote in the introduction to his **Course in Elementary Practical Physiology**, 'Histological work unless it be salted with the salt either of physiological or of morphological ideas, is apt to degenerate into a learned trifling of the very worst description ..... In morphological questions the physiologist has but an indirect interest; and the details of microscopic structure ought only to occupy his attention in so far as they serve as a basis for physiological deductions.'<sup>145</sup>

It became increasingly obvious in the 1870s that what **should** occupy the physiologist's attention was, above all, the chemistry of the organism. In part, this was only a shift in emphasis: earlier writers, like Todd and Bowman, had allowed space to the chemical composition of the body in their textbooks.<sup>146</sup> Because of their stress upon the formed cell as the true organic agent, however, Todd and Bowman, and the school they represented, had given less emphasis to these substances than to the structures through which they passed. In contrast, the trend of the 1870s was to view animal life as 'an aggregate of chemical processes for which food and oxygen afford materials, the products being heat, muscular action, carbonic anhydride, water and ammonia.' The seat of these actions was the protoplasm; that is, the substance which 'forms the active parts of the tissues' and which 'exhibits itself in all the general actions which belong to the organism as a whole.'<sup>147</sup>



This did not rule out the study of particular organs to which some part of the general potency of the protoplasm had been delegated. However, even such investigation of 'the specific actions of particular parts' was to be conducted in terms of the chemical and physical changes they exhibited, with the emphasis upon the chemical. For example, Burdon Sanderson's account of intestinal digestion was a discussion of the action of the pancreatic fluids and other chemical agents upon the food.<sup>148</sup> There was no morphological detail, in marked contrast to texts of twenty years before where the structure of the intestinal villi had provided the main focus of attention. Attached to this instance of the new style of physiological analysis was a set of 'practical exercises', chiefly involving chemical experiments on foodstuffs and digestion.<sup>149</sup>

This approach to physiology, with its indifference to morphology and its experimental bent, received a definitive statement in Foster's *Textbook of Physiology* (1877). There the amoeba, an organism 'wholly or almost wholly composed of undifferentiated protoplasm', was taken as the type of life. In the amoeba 'the problems of vitality are reduced to their simplest forms.' It moved by means of the continuous motion of its protoplasm; it digested by the protoplasm's absorption of food; it reproduced by a division of the protoplasm.<sup>150</sup> These changes were basically identical to muscular contraction, digestion and reproduction in higher animals. Similarly 'nutrition', or as Foster preferred to call it, 'metabolism', was a function of constitutive chemical powers of the protoplasm throughout the organic world.<sup>151</sup>

Foster did not, however, rely exclusively upon the chemical properties of protoplasm to explain function. He recognised that in the higher life-forms, the protoplasm became differentiated into gross structures whose action could be regarded mechanically. Thus, in his discussion of the function of the kidneys, he included four elements: 1) the chemical composition of the urine; 2) the relation of secretion to arterial pressure; 3) the correlation between excretion through the skin and through the kidneys; and 4) nervous control of the bladder.<sup>152</sup>

Generally, he conceded that metabolism could be regarded both as the chemical process whereby food was oxidised and otherwise adapted to the body's needs; or mechanically, as the conversion of potential into usable energy. To illustrate the latter form of physiological analysis, Foster gave a table of the thermal and mechanical equivalents of unit-weights of different foods.<sup>153</sup>

Nonetheless, Foster's emphasis remained on the chemistry of life; it was along these lines that he saw the most promising forms of research developing. The 'whole secret of life', he declared, 'may almost be said to be wrapped up in the occult properties of certain nitrogen compounds ..... If we admit that the energy of muscular contraction (and with it the energy of all other vital manifestations) arises from an explosive decomposition of a complex substance, which we may call real protoplasm, and that this complex protoplasm is capable of reconstruction within limits which ..... may be very wide, we acquire a conception of physiological processes which, if not precise and definite, is at least simple and consistent, and moreover a first step towards a future molecular physiology'.<sup>154</sup>

By the later 1880s the groundwork of such a 'molecular physiology' or, as it came to be called after 1900, of 'biochemistry' were laid. Ironically, however, the triumph of this approach involved the eventual abandonment of 'protoplasm' as a useful notion and the reintroduction of structural concepts into physiology.<sup>155</sup> This transition was evident in Burdon Sanderson's address to the Biology Section of the 1889 BAAS at Newcastle. There the antithesis between structural and chemical explanation had virtually vanished; in place of a facile appeal to that 'worn-out *Deus ex machina*', protoplasm, Burdon Sanderson advocated the presumption that some form of invisible organisation corresponded to each function. It was necessary to conceive of the body as, in the final analysis, 'framework' and 'context,' or as 'channel and stream'; as in Cuvier's vortex theory, the transient substance of life passed through a stable structure. Specifically, Burdon Sanderson identified the fixed framework of the sub-microscopic structure of the body with the ferments or enzymes, which altered other substances without themselves entering into the reaction, and which executed the chemical changes which underlay all organic processes.<sup>156</sup>

Foster's emphasis upon the chemistry of life was not, however, related solely to the line of research which was most likely to yield results. The ideal of experimental physiology could only be realised within a certain institutional framework; to attain this, the advocates of such physiology were obliged to enter the market-place as vendors of a commodity to be valued. In particular, they had to present their product in a way which would, it was hoped, appeal to their most likely customers.

The main aim was to secure the means to equip and to staff physiological laboratories. In 1870 H.P. Bowditch complained that 'in England we have absolutely no physiological laboratories open to

students', which were comparable to the great German institutions.<sup>157</sup> He was corrected by Burdon Sanderson who pointed out that there were physiological laboratories at University College London and at Edinburgh. Burdon Sanderson chose rather to stress the absence of opportunities for physiological workers.<sup>158</sup> In fact, the laboratory which Burdon Sanderson set up in London in 1870 was 'nothing much more than a room over a stable'. In the following year University College, thanks to a bequest, was able to found the Brown Institute which contained a pathological laboratory. Burdon Sanderson abandoned his previous establishment and took over these facilities, and began building a 'school' on the German model.<sup>159</sup>

A similar development took place in Cambridge after 1870. Michael Foster took up a post at Trinity College and began the arduous task of establishing physiology in a basically hostile environment.<sup>160</sup> From the first, he made laboratory work a very large part of his course of physiological instruction. Lectures alone, 'he did not regard as a very fructifying mode of sowing seed. He thought it far better that men should work and see for themselves.'<sup>161</sup> For this purpose he demanded facilities where such training as well as original research could take place.

However, both Burdon Sanderson and Foster realised that the success of these efforts to establish experimental physiology depended upon the recognition of the subject as an integral part of medical education. The growth of physiology in Britain was retarded by 'the inability of physicians and surgeons to recognise its status and value, as distinct from anatomy'.<sup>162</sup> The German physiological laboratories had grown up as adjuncts to the medical schools in Berlin, Leipzig, Vienna and elsewhere; as a result of this patronage of the science by medicine, a true physiological profession had developed: students had the opportunity to specialise in physiology and were subsequently able to support themselves in this pursuit by university teaching. In Britain, in contrast, there were 'very few investigators by profession, and, in the [German] ..... sense, no "Schools" '. The few who had acquired some physiological competence through their own efforts were usually forced to abandon research and to gain a living by medical practice.<sup>163</sup>

The professionalisation of physiology in Britain was seen to depend upon the creation of a comparable relation between the science and medicine as existed on the Continent. As Burdon Sanderson told the Devonshire Commission in 1874: 'I believe that in all countries, and at all times, physiological

research must be conducted by people who are more or less connected with the medical profession.' A physiological laboratory that was detached from a medical school would fail 'for want of people to work in it'.<sup>164</sup> Specifically, Burdon Sanderson aimed at the style of social organisation of the discipline which was found in Germany. In this, there were two kinds of laboratory worker: the first, the class of professional teachers of physiology, was virtually unknown in Britain; similarly, there was no counterpart of the 'student workers', usually trainee, or recently qualified, physicians who chose to spend a few years in research at the German universities. In Britain, Burdon Sanderson opined, 'if physiology is ever to be carried out successfully, it must be by the same agency, a combination of students and teachers of medicine.'<sup>165</sup>

Although Burdon Sanderson acknowledged that most of these students would finally go on to medical practice, he expected that 'the best and most able of those young men would become teachers, and would occupy themselves entirely [with physiology] ....., and would enter the profession of science, and not the profession of medicine.'<sup>166</sup> Thereby, the physiological schools would perpetuate themselves. As Huxley frankly told Lankester in 1890, 'Our side has been too apt to look on the medical schools as the feeders for science'.<sup>167</sup> Not only was medicine to supply the funds necessary to experimental physiology, but a proportion of medical graduates passing through the laboratories was to be regularly creamed off to provide the next generation of researchers.

Since medicine was the most likely patron for physiology, it was necessary to adapt the content of the science to the supposed needs of the trainee physician. In practice, this meant a concentration upon organic chemistry, rather than upon more *recherché* topics like body electricity and the nervous system. Thus Foster excused the neglect of such topics in his textbook on the grounds that 'I am writing chiefly for students of medicine'. Indeed, the desire 'to contribute to the development of physiology in the medical profession, has ..... been my guiding principle in writing this book'.<sup>168</sup>

Seven years earlier, Huxley had foreshadowed this emphasis upon chemistry as the aspect of physiology most relevant to the medical student in a memorandum on the place of physiology in the medical curriculum of University College. The course that he proposed was predominantly concerned with the chemical composition of the various tissues, of food, of the blood and lymph fluids, and of the excreta. He also included the general nature of digestion and the physical and chemical processes

involved in respiration.<sup>169</sup>

Huxley later justified this bias in terms of the supposed relevance of organic chemistry to pathology and to therapeutics. He observed in 1881 that pathological science had long followed the path initiated by Bichat; that is, to make an ever more minute study of structure, in order to identify the deformities characteristic of disease. But histology had proceeded along this path as far as it could: it was 'a science of yesterday'.<sup>170</sup> Moreover, the knowledge that had so been gained of morbid structures had produced no effective therapy. Huxley argued that to know **what** (in the anatomical sense) happened during disease was not enough; it was also necessary to know **why** (in the physiological sense) it happened. Until the pathologist had a clear conception of 'the chain of causation' involved in disease, he had no chance of influencing those events; an effective pathology waited, therefore, upon an adequate physiology.<sup>171</sup>

Until recently the 'general conceptions' of physiology had been erroneous, and the science as a result had been of little medical use. But now that the subject had proceeded beyond its previous histological preoccupations, it should no longer be despised by the physician. The fundamental model of life currently entertained by the physiologist was of 'a vast aggregate of molecular mechanisms performing complicated movements of immense rapidity, and sensitively adjusting themselves to every change in the surrounding world.' On this view,

pathology is the analogue of the theory of perturbations in astronomy, and therapeutics resolves itself in to the discovery of the means by which a system of forces competent to eliminate any given perturbation may be introduced into the economy.

In effect, the 'means' by which Huxley proposed to rectify organic perturbations were **pharmacological**; the physiological phenomena of most relevance to the pathologist were, therefore, **chemical**.<sup>172</sup>

Because vitality was essentially a sum of chemical processes, so disease must be the result of some abnormality in body chemistry; it followed that effective therapy must depend upon the introduction of chemical agents able to reverse the morbid tendency. Given an adequate knowledge of organic chemistry, Huxley predicted,

sooner or later, the pharmacologist will supply the physician with the means of affecting, in any desired sense, the functions of any physiological element of the body. It will, in

short, become possible to introduce into the economy a molecular mechanism which, like a very cunningly-contrived torpedo, shall find its way into some particular group of living elements and cause an explosion among them, leaving the rest untouched.<sup>173</sup>

If chemical physiology offered such power to the physician it could not be ignored in medical education. On the contrary, 'the future of pathology and of therapeutics, and, therefore, that of practical medicine, depends upon the extent to which those who occupy themselves with these subjects are trained in the methods and impregnated with the fundamental truths of biology.'<sup>174</sup> It is important to stress, however, the degree to which such utterances were pure rhetoric. The instrumental value of organic chemistry to medicine between 1860 and 1880 was minimal. Such investigation into pathological chemistry as had taken place was confined almost entirely to the effects of **exogenous** toxins.<sup>175</sup> Huxley's vision of a specific for every disease was pure fantasy.

His address did, however, accurately record the polemical goals which underlay the stress upon the chemistry of life in contemporary physiological literature and pedagogy. The medical profession, and the resources it controlled, were the target of this bias, in that the emphasis upon the chemical was seen as the line most likely to make the physicians yield that which the physiologists needed: money and jobs.

In general, the concept of life as a function of substance was deployed as part of a strategy of professionalisation. The view that life was the result of a complex of physico-chemical changes negated vitalism and avoided the metaphysical and theological involvements of that theory. Further, it reversed the excessive stress upon the methodological peculiarity of physiology which had followed from the doctrine that a special organisation, to be discerned by the microscope, underlay vitality. 'Substantialism', in contrast, stressed the applicability of both the conceptual and the technical resources of the physical and chemical sciences to physiology. In particular, it furthered the interest in establishing physiological laboratories, with all that implied for the social structure of the discipline, as the proper setting for investigation. Finally, through the alleged relevance of chemical physiology to medicine, the substantialist theory of life contributed to the polemic whereby resources for physiology were sought in mid-nineteenth century Britain.

However, an emphasis upon this theory of life and upon this set of interests should not obscure

the diversity of life theories in mid-Victorian Britain, nor the variety of interests that they served. In particular, between 1860 and 1880, there was a revival of vitalism in Britain which, despite the contemporary tendency towards professionalism, deliberately replaced the question of life in the context of broader philosophical concerns. At the same time, the physicalism of such as T.H. Huxley also served a general as well as a more special interest. That theory of life too, was implicated in a comprehensive world-view, and was consequently the object of widespread controversy.

#### iv. "New" Vitalism

Debates about the character of the organism were not confined to learned journals and textbooks of physiology in Victorian Britain. They also took place in general periodicals, newspapers, pamphlets, and in public addresses. This widespread interest in vitality during the 1860s and 1870s was, in large part, due to the political significance of various cosmologies during this period. In particular, a naturalistic outlook, with its implicit criticism of conventional Christian notions of the relation of God to nature, was a part of the contemporary strategy of 'Advanced Liberalism'.<sup>176</sup>

In the course of these controversies many of the features of previous attempts to use conceptions of life to make political points recurred. The British context was also in many ways analogous to events in contemporary Germany. In Germany left wing political factions similarly identified the Church and its dogmas among the obstacles to radical change. Polemicists like Vogt, Moleschott and Buchner resorted to materialism as a means of discrediting the world-view of their opponents. The body played an especially important part in their rhetoric: it provided them with a focus for their claim that matter and energy alone were the basis of all physical phenomena. If life could be shown to depend only upon these conditions, then a crucial area of cosmology would be lost to the spiritualists.<sup>177</sup>

In the same ways, their British counterparts tried to deny that the living world was permeated with spiritual as well as physical causes. Their aim was to assert that no fundamental distinction existed between life and the rest of nature. As Huxley said in one of his 'Lectures to Working Men', the difference between the organic and inorganic worlds was slight, and arose from 'the diverse combination and disposition of identical forces, and not from any primary diversity'.<sup>178</sup>

In contrast, the defenders of orthodoxy chose to strengthen the barriers between living and non-living. In consequence, the mid-nineteenth century saw the resurgence of the type of vitalism which had been current thirty years before. Both the Edinburgh and the London histological schools saw, by the 1860s, their leader adopt a brand of animism.

Goodsir became increasingly concerned with the theological implications of trend towards naturalism in biology in the period after 1859. He was hostile to the Darwinian theory and to Huxley's view on man's place in nature. In his own 'anatomical and medical inquiries he had always stood out



for man's superiority in the scale of being, his high attributes and spiritual relations'.<sup>179</sup> One way of protecting that **scale** of being, of insisting that their **were** absolute distinctions in nature, was to maintain that living and non-living were fundamentally different and that a spiritual principle was needed to explain the discrepancy between them.

In 1856 Goodsir had reacted against the reduction of vital to inorganic which Vogt in Germany and Spencer in Britain had proposed. He noted with alarm that the notion that 'mind is a product or a function of the matter of the organism' was gaining ground. This was, he argued to reverse the proper precedence: not only was the human consciousness irreducible to any material arrangement of the nervous system, but the organisation of the matter of the body into cells, fibres and organs implied the existence of a further psychic principle. Each organism, in addition to its material forces, possessed 'a distinct essence' which ordered the former.<sup>180</sup> In effect, Goodsir advocated the existence of an 'anima' as well as a 'mens' in the body.

He elaborated this view in his lectures to the Edinburgh anatomy class in 1862. Then he argued that the psychic element in the animal was 'virtually the animal itself; for it is that, failing which the body of the animal would have no existence ..... the structure of the animal is merely the instrument of its instinctive consciousness'.<sup>181</sup> This consciousness was the **cause** of the form and function of the organism; the 'immaterial principle' was therefore paramount over matter.<sup>182</sup>

Goodsir used the vital principle to launch an attack upon naturalistic conceptions of man. Each animal soul was specific and immutable; this vitiated the notion of the transmutation of species and, in particular, that of the descent of man from lower forms of life. Further, the idea of an 'indwelling psyche' upon which the processes of life depended ruled out the **reduction** of the mental to the physical and discredited the claim that mind was a natural product. As in Abernethy's scheme, the vital principle formed a barrier between the mental and the physical. All 'trustworthy' investigators were agreed, Goodsir alleged, 'that man in his constitution consists of three elements — a corporeal, a psychical, and a spiritual.' Moreover, a definite hierarchy existed between these human components; upon the psyche depended 'that determinate and co-ordinated action of all those physico-chemical forces which are collectively engaged in the development of the body from the ovum, and in his life-long structural modifications and physiological actions'.<sup>183</sup> While naturalism collapsed the mind-

matter boundary, therefore, vitalism reinforced it.

Goodsir advocated a cultural role for scientific knowledge that was as contrary to the views of the scientific naturalists. He insisted upon the theological implications of cosmology and required that science defer to Christian dogma on such questions as the nature of vitality. He told his class that science was a product of the Christian outlook, but that 'one of the greatest dangers to which the Christian system is at present exposed, is the erroneous tendency to elevate science above the other forms of belief'. He mentioned this, Goodsir added, to make clearer the significance of his assertion that the human body was 'an instrumentality under the guidance of the human soul towards the end for which man was placed on this globe'.<sup>184</sup> Just as matter relied on spirit to give it form and motion, so did science rely on theology to provide its ultimate interpretative framework.

These arguments were echoed by students of Goodsir like H. Alleyne Nicholson<sup>185</sup>, and became part of the weaponry of the theological opponents of naturalism, especially in Edinburgh itself. In consequence, when in 1868 Huxley went there to address a kirk congregation on the nature of life, he was assaulting both a rival view of the organism and an inimical conception of the relationship which should exist between science and theology. The paper which he gave, 'On the Physical Basis of Life', was crudely polemical: Huxley's aim was to shock, and, in this, he succeeded. The effect of the piece was to draw further attention to the question of vitality and to polarise opinions about it.

The impact of the talk was extended when it was published in the **Fortnightly Review**; as John Morley remarked, 'No article that had appeared in any periodical for a generation back ..... excited so profound a sensation as Huxley's memorable paper On the Physical Basis of Life'. At its widest, the article was seen not merely as a piece of popular physiology with theological overtones, but as a political statement. When 'all this free-spoken and extremely competent dissent from orthodoxy came to be found in company with ideas on social and political renovation of various sorts' in the pages of the **Fortnightly**, 'the combination awoke a trifle of discomfort in the old hands of the political world.'<sup>186</sup>

Huxley's goal in 'On the Physical Basis of Life' was to provide the strongest possible contrast to the vitalist position. He compared the notion that there was a physical basis for life with the widespread misconception that life was something 'which works through matter', but is independent of it.<sup>187</sup> He

referred all vitality to the manifold properties of protoplasm, and denied any basic difference in this respect between the infusorian and the higher animals including man. All that changed in the course of evolution was the distribution of vital power, not the nature of that potency itself.<sup>188</sup> Nor was there any real divide between the organic and the inorganic worlds: vitality was a result of the nitrogenous elements of protoplasm combining in a certain way, just as the properties of water were the consequences of a particular compound of oxygen and hydrogen. It was as inappropriate to hypothesise a mysterious 'vitality', which presided over the body, as it was to imagine that something called 'aquosity' entered into the oxidated hydrogen and endowed it with wateriness.<sup>189</sup>

Once this dependence of function upon matter had been conceded, then it was easy to make a similar inference about mind itself. In the same way that vital actions were the properties of vital force, 'the thoughts to which I am now giving utterance, and your thoughts regarding them, are the expression of molecular changes in the matter of life.'<sup>190</sup> Far from the psychic being separate and dominant, therefore, it was assimilated to the other vital phenomena and subordinated to matter.

Huxley denied, however, that he was a materialist. The **propositions** he had used were indeed materialistic, but, given the limitations upon human knowledge, these could have no absolute ontological status. Rather, materialism in physiology was to be justified on the pragmatic grounds that it brought the science into a closer relation with physics and chemistry, disciplines which had generated much instrumentally powerful knowledge. It was reasonable to assume that a materialistic terminology in physiology might 'in future, help us to exercise the same kind of control over the world of thought, as we already possess in respect of the material world; whereas, the alternative, or spiritualistic, terminology is utterly barren, and leads to nothing but obscurity and confusion of ideas.'<sup>191</sup>

As so often in the polemic of scientific naturalism, materialism shaded into positivism. Thereby, the basic cosmological doctrines that were desired were secured without any strong, easily criticised, ontological commitment. Moreover, as Huxley's concluding remarks show, positivism provided an ideal idiom for articulating the goals and assumptions of an autonomous science that was unencumbered by metaphysical baggage. On this view, the aim of science was merely to predict and to control phenomena; questions of the nature of things in themselves were irrelevant.

John Tyndall, who also produced polemical pronouncements on the nature of life in the late 1860s and early 1870s, was less inclined to take refuge in such positivism. Like Huxley, he stressed the dependence of the vital and the psychic upon the material, but with a different emphasis. Tyndall extended the point from the body to the universe in general.

Tyndall took advantage of his address to the Mathematics and Physics Section of the 1868 BAAS at Norwich to discuss the character of formative agency in nature. He distinguished two types of constructive power: there were, on the one hand human constructions like the pyramids which were the result of an **external** agency; on the other hand, were natural structures like crystals. Tyndall denied that these were built on the same principle as man-made edifices: he repudiated the view that the molecules of a crystal were 'an invisible population, controlled by some invisible master, placing the atomic blocks in position'. The 'scientific' view, he argued, was that the potential for producing organisation was **intrinsic** to the molecules: the 'molecular blocks ..... are self-positing, being fixed in their places by inherent forces with which they act upon each other.'<sup>192</sup>

Given this principle, there was no more need to assume an external force at work in a living structure. Organic molecules, too, were 'positing by the forces with which they act each upon the other.'<sup>193</sup> Vitality was thus assimilated into a general conception of natural agency.

Tyndall expatiated upon this outlook in a discourse to the Liverpool BAAS in 1870. At issue, he argued, was whether life had to be conceived as something added to matter; by 'life' he meant activity and power in general. The theory that 'life' was something extrinsic to matter was compatible with an essentially dualist cosmology in which the motive and directing principle, spirit or mind, was distinguished from the moved substance, matter. The two, spirit and matter, had Tyndall held, 'ever been presented to us in the rudest contrast, the one as all-noble, the other as all-vile'. However, he questioned this crude separation: instead of this antithesis of spirit and matter, they should be regarded as 'equally worthy, and equally wonderful', as, in fact, 'two opposite faces of the self-same mystery'.<sup>194</sup> Tyndall commended a cosmology which denied that there were two or more substances in nature, but which identified mind, life, and extension as properties of a single substance. In other words, he urged men 'to look upon matter, not as "brute matter", but as the "living garment of God"'.<sup>195</sup>

It was this contribution towards hylozoism, panpsychism and, finally, an identification of God

with nature, that struck many contemporaries as the most salient feature of the naturalistic theory of life. As a result Huxley and Tyndall's utterances elicited a mass of criticism, much of which was directed to maintaining the categorical distinctions between life-mind and body, and so between God and matter. Edinburgh, again, was a major source of such polemic. By 1868, John Goodsir was dead; but his brother, Joseph, a cleric, was present at Huxley's discourse and published attacks upon this and upon Tyndall's 'Scientific Materialism'.

According to Goodsir, the fundamental question raised by scientific naturalism was: 'Does a living being, say man, consist of two distinct and totally different substances', matter and spirit, or of only one? In place of Huxley's epistemological 'scepticism', which Goodsir linked with Hume, Goodsir placed himself 'on the ground of realism, behind the bulwark against scepticism reared by Reid [and] Hamilton'.<sup>196</sup> Upon realist premisses it was both possible and necessary to go beyond the phenomenal and to ask questions about the 'matrix' which must be assumed to explain events: for example, to explain the forms taken by protoplasm in the organism. Goodsir concluded that 'the real and universal matrix of all this must be an ultimate, omnipresent, omnipotent, all-wise, all-knowing Being'. Such a 'fountain and matrix' of the order and power in nature needed essentially psychic characteristics and must, therefore, be spiritual; it was necessary to look beyond matter to 'the infinite and eternal Ego, ..... [which] appears, producing, ordering, co-ordinating, and sustaining all things as a systematic whole.'<sup>197</sup>

Another Edinburgh philosopher, James Hutchinson Stirling, tended to look to Kant and Hegel, rather than to Reid and Hamilton, for resources to bring against the naturalistic theory of life. However, the end-product of his argument was also to claim that spirit was independent of and superior to matter. Stirling paid less attention to Huxley's epistemology than to his physiology: he recognised that protoplasm was the outcome of a gradual dissolution of the notion of the cell as a morphological unit of life, but argued that Huxley had gone too far in denying the relevance of structural concepts to the understanding of the organism. Stirling preferred Virchow's and John Goodsir's view that the cell was not an 'accidental cavity', created in the course of evolution by the random play of mindless forces; rather, the organism displayed a form which 'could have been constructed, was constructed, only in reason, and by reason.'<sup>198</sup> Amorphousness implied chance; on the other hand, Stirling argued, structure implied the design and control of the material elements of the body by an intelligent agent.

Of all the organised parts of the body, the cell illustrated the nature of vital action most clearly. It was more than a structure: it was an *agent*, a single entity which executed a variety of functions. Thereby, Stirling argued, it revealed the 'subjective' nature of organic processes; that is, it showed the necessity of referring life to an individual, quasi-psychic power, rather than to the general 'laws of matter'. In effect, Stirling identified the cell as the locus of a teleological principle, which belonged to an order other than that occupied by the physical constituents that it manipulated. The vital force implied 'a new world — a new and higher world, the world of self-realising thought, the world of an *entelechy*.'<sup>199</sup>

The debate between naturalistic and anti-naturalistic theories of life therefore possessed a general form reminiscent of that of past cosmological controversies. On the one hand, there was a hierarchy-reducing strategy; on the other, a defence of hierarchy. While the one advocated a monism in which matter was omni-potent, and capable of generating both life and mind; the other stabilised the distinctions between those categories and asserted the sovereignty of mind. The reductionist strategy can be related to the general goals of scientific naturalism: namely, to the attempts to establish a materialistic idiom of scientific explanation which excluded 'teleology' as an explanatory resource; and to the additional role of naturalism in the rhetoric of political radicalism in mid-Victorian Britain. Both interests involved the derogation of spirit. If God acted, immediately or mediately, upon the organism or elsewhere in nature, then 'design' and the other devices of natural theology could not be eliminated from scientific explanation. Similarly, the bias of 'advanced thought' required that theism be systematically discredited; because vital properties had been presented as displays of divine action, a purely materialistic concept of life served this purpose.

Conversely, vitalism, and its associated ontology, was an attempt to defend the dogmas of Christian orthodoxy, and, more especially, to sustain the project of a natural theology. From the theological point of view, the organism was more than a collection of chemical and physical processes; it was a means of showing the power and providence of God. In the words of one divine,

In the presence of the meanest plant, the merest animalcule, we stand before an awful and inscrutable mystery. Oh life, life! what, whence art thou? Self-motion, self-growth, self-perpetuation! It is Almightiness! It is God! It is the power of God! It is God working!<sup>200</sup>

This use of the organism involved a stress of the dependence of nature upon super-nature: upon the notion that 'properties, powers, self-activity, can belong only to a being, not to a thing — in the highest analysis, to mind not to matter.'<sup>201</sup>

The kind of cosmic order needed for natural theology of this sort was, therefore, the exact opposite of that embodied in the world-view of scientific naturalism. While the latter sought to lodge all power in matter; the former insisted on the existence of a transcendent spiritual principle without whose action matter was inert and inchoate. Underlying this conceptual difference was a conflict between the old-established and the parvenu professions. Some clerics resented the claim to the autonomy of natural knowledge that lay at the heart of naturalism; they wished to reassert their traditional privilege to condition natural philosophy with theological considerations. According to John Young, science was 'emphatically the record of Divine physical providence, it is the discovery and announcement of that fixed course, according to which the Great Being has chosen, and chooses to act, in all the spheres of material nature'. For scientists to concentrate solely upon the material at the expense of the spiritual 'must be dangerous, if not fatal to truth'.<sup>202</sup>

From this perspective, Huxley's account of life stood condemned. Its author had ignored the 'postulate of all postulates' upon which science should proceed: the existence of a 'God and Creator, Fashioner, and Ruler of the material universe'. More, Huxley represented a tendency to identify naturalistic science as the only form of valid knowledge. Like his mentor Hume he seemed to imply that only the empirically ascertainable could count as knowledge; this was a challenge to those who earned a living by developing other areas of culture. Why should 'science disparage all other studies in comparison with itself? Why should it aspire to be the highest, even the only real power on earth?' The claim that over and beyond matter was spirit corresponded to the assertion that in addition to science there was a 'high and spiritual philosophy'; just as there was something more powerful than physical force, so there was 'something higher and mightier ..... than science'. It was necessary to supplement belief in natural phenomena with belief in 'a living God' and in a 'living human soul'. This spiritual realm was as much a field of possible knowledge as the matter of science.<sup>203</sup>

It was not only theologians who felt threatened by the aggressive naturalism of Huxley and his like. There were scientists who also set themselves against this trend. Notably, Lionel Beale waged a

long campaign against the protoplasmic theory of life. This resistance needs to be referred in part to Beale's professional competences. Beale was a master-microscopist who saw his kind of work belittled by the advocates of experimental physiology in the 1860s and 1870s. But wider interests were also involved: Beale operated out of the avowedly Anglican King's College London which had been established to counter the 'infidel' influence of University College; he was, moreover, interested in the theological implications of physiology and a familiar of such anti-naturalist divines as Henry Wace. Tyndall combined these factors when he dismissed Beale's opposition to the physicalist theory of life as that of a 'microscopist, ignorant alike of philosophy and biology', who belonged to a College 'famous for its orthodoxy'.<sup>204</sup>

Beale's commitment to natural theology extended into the details of his histology in the period 1860 - 1878. He claimed to have produced visible evidence of a vital principle in living tissue. In so doing, Beale was merely glossing 'facts' at which he had arrived previously. In 1861 he had detected two types of matter in the organism: the active or 'germinal' matter and the passive or 'formed' matter. Only the first, which was the real agent of organic processes, was truly 'alive'; formed matter was merely its instrument and residue. The two matters could be distinguished by their different responses to carmine-staining: while the germinal matter took the dye, the formed matter remained colourless.<sup>205</sup>

By 1865 Beale was no longer satisfied with a bare statement of these facts. He told his class in physiology that in recent years the organism had become an object of public debate; lest a materialistic interpretation should win by default, he felt obliged to present his own interpretation.<sup>206</sup> The distinction between formed and germinal matter, he argued, corresponded to that between the seat of the ordinary physical forces in the body and that of the special vital force. The germinal matter possessed a 'mysterious agency' which acted upon ordinary matter and regulated its forces. The vital power was 'as different from the force as it is from the matter. It is one of the immaterial agencies in Nature'.<sup>207</sup>

In the aftermath of Huxley's 1868 address, Beale expanded upon this scheme at great length. He repeatedly claimed that ordinary matter could never achieve organic form; a psychic, spiritual element had to be added. The existence of this power was itself evidence of 'the activity of an agency perhaps related to vitality, but of a yet higher order'. This superior agent was 'capable of influencing,



controlling, and directing not only living power, but all matter and all force of whatever kind.'

Thus, he concluded, 'the Theistic idea presents itself to the scientific imagination.'<sup>208</sup>

According to Beale this assertion was not based upon mere speculation. On the contrary, it was an inference from 'the most minute investigation into the structure and actions of living beings'.<sup>209</sup> When 'properly' treated, the germinal matter was revealed to the microscopist as the true source of vitality whose movements were inexplicable in purely physical terms. It was therefore necessary to assume the action of an additional power; and, thereby, access was gained, **through science**, to a world beyond nature in which God and the soul could be safely stored.

In Beale the skills of the microscopist were turned to the service of natural theology and against the physicalism which both denigrated histology and which rejected the search for divine agency in the organism. As late as 1899, Beale continued to appeal both for a theological dimension to biological thought and for a greater respect for microscopical studies. Indeed, he seemed to suggest that the latter was best guarantee of the former: he wrote that microscopical study had enabled many 'to see through physico-chemical, agnostic, anti-biological vagaries'.<sup>210</sup> By this time, however, vitalism was receiving support from many quarters: its articulation had become part of the general 'reaction against scientific naturalism' of the period.

For the sake of completeness it is worth noting that physicalist and vitalistic theories did not monopolise the philosophy of the organism in Victorian Britain. During the 1870s G.H. Lewes argued for a third option which mediated between the two extremes. His 'organicism' recognised, with vitalism, the unique character of living processes; but it nonetheless retained the ideal of physico-chemical explanation. The distinctive character of life lay, not in the action of some hyper-physical agent, but in the **relations** into which material elements were combined in the organism.<sup>211</sup>

By the early twentieth century such organicism was an important doctrine in physiology, embryology and cytology; it was also a much-used resource in philosophy and social thought. However, Lewes's position remained an isolated statement in mid-Victorian Britain because it could mobilise the support of no important interest. In contrast, physicalism served the interest of the professionalisation of physiology and formed part of the radical polemic of the period. Vitalism presented a view of the body compatible with the aims of natural theology. Moreover, during the

1880s and 1890s it became an aspect of a 'conservative' philosophy of nature.<sup>212</sup>

The controversy between physicalism and vitalism turned ultimately upon the status of mind in nature. This preoccupation ensured that those physiological functions most directly connected to mentality, those of the nervous system, were objects of particular concern. In addition, the period 1840 - 1880 saw the development of more specialised interests in neurology and psychology. These events form the focus of the next chapter.

## CHAPTER FOUR: Mind and Nature

### Introduction

In a sense, the debates about causation and life discussed above were also controversies about mind.

A basic issue in them was whether the direction evident in natural events needed to be attributed to the agency of a psychic being or whether the 'laws of matter' were sufficient to explain all occurrences.

The manner in which analogies were drawn between the body and the universe in these arguments has been noted. In discussions of human psychology the relation of mind to its physiological correlates was also suffused with a broader significance: a dense two-way traffic passed between the human in particular and the natural in general. In these circumstances there could be no firm demarcations between psychology, physiology, theology, and metaphysics; discourse in each passed into the rest.

At the same time, however, more specialised interests in the nervous system began to appear. From certain points of view the body was not, primarily, the exemplar of great cosmological truths, but an object of professional concern to be classified and manipulated for relatively narrow purposes. In these contexts the way in which the relation of the spiritual to the physical was designed had immediate practical implications, whether by implying a mental pathology and therefore a therapeutics, or by suggesting lines of scientific enquiry into the physiology of mind.

The object of this chapter is to trace these various themes in the mental philosophy and neurology of nineteenth century Britain. In particular, it considers the way in which a certain model of the nervous system, one which reversed the conventional matter-spirit hierarchy, was constructed in the 1830s and 1840s, and then put to a number of uses. This will lead into a consideration of the part that this neurology and its associated metaphysics played in the general strategy of scientific naturalism in Victorian Britain.

### i. Reflexion, Ganglia, and Man's Immortal Soul

The 'prevailing doctrine of the anatomical schools' in Britain during the second decade of the nineteenth century was, according to Charles Bell, that 'the whole brain is a common sensorium; and that the extremities of the nerves are organised, so that each is fitted to receive a peculiar impression; or that they are distinguished from each other only by delicacy of structure, and by a corresponding delicacy of sensation.'<sup>1</sup> The various impressions received at the extremities of this unified system of nerves were carried along their length to the brain, 'and presented to the mind; and ..... the mind by the same nerves which receive sensation, sends out the mandates of the will to the moving parts of the body.' In addition to the cerebral nerves, it was held that there was 'a set of nerves, called vital nerves, which are less strictly connected with the sensorium, or which have in them knots, cutting off the source of sensation, and thereby excluding the vital motions from the government of the will.'<sup>2</sup>

Several points about this view of the nervous system deserve notice. First, the brain (or 'encephalon') was regarded as a **unitary** structure, which was also functionally undifferentiated. In its entirety, the brain was 'the common sensorium' — ie. the organ whereby the immaterial mind received impressions and issued the volitions which occasioned bodily movement. Second, the encephalon was the **only** centre in the nervous system that was so capable of assimilating sensory information and initiating action; sensori-motor potency and consciousness were therefore necessarily intertwined. Third, it followed that the rest of the nervous system performed a purely internuncial role — transmitting impulses to and from the brain. The nerves, too, were uniform structurally and functionally, and differed only in an unequal sensitivity at their distal ends to certain kinds of impression. The same nerves were assumed to carry messages both to and from the brain. Fourth, the existence of other nervous structures, organised upon radically different principles, was recognised: the 'vital' nerves, unlike those of the central nervous system, had no anatomical connection with the cerebrum; as a result, they were independent of the conscious control of the mind. The impulses which travelled along them were cut off from the common sensorium by the 'knots' that punctuated the course of the vital nerves. These knots (usually referred to by their Latin name 'ganglia') were, therefore, by implication, centres which regulated the vital motions of those organs that operated independently of the will.<sup>3</sup>

Between 1820 and 1850 most of these doctrines were subverted, and an alternative view of the structure and function of the nervous system widely accepted. On the anatomical side, structural complexity and differentiation was substituted for the unitary view of brain and nerves; physiologically, the sole dominance of the encephalon was challenged, and a mode of nervous action independent of consciousness and will developed. The model of the 'vital' nervous system provided important structural and functional resources that facilitated the overthrow of the old view of neural organisation.

Bell's 'new anatomy' of the brain and nerves formed one aspect of this revolution. In 1811 he criticised the prevalent simplistic view of these organs and the identification of the whole encephalon as the seat of consciousness. Bell advanced the alternative theory that the brain was an agglomeration of organs, which were diverse in function as well as in structure; consciousness he confined to the highest of these organs, the cerebral hemispheres.<sup>4</sup> Further, he proposed the doctrine which was to become the most closely associated with his name, that

The nerves which we trace in the body are not single nerves possessing various powers, but bundles of different nerves, whose filaments are united for the convenience of distribution, but which are distinct in office, as they are in origin from the brain.<sup>5</sup>

Bell held each spinal nerve to have two roots: one of which, the posterior, was *sensory* in its office — it transmitted external stimuli toward one part of the brain — while the other, anterior root performed a *motor* function — it carried impulses from another part of the brain to the muscles.<sup>6</sup>

Although initially ignored in Britain, Bell's view of the nervous system received during the 1820s and 1830s powerful indirect support from Continental neurology. His conception of the brain as a terraced structure, in which functionally discrete organs were superimposed on one another, was compatible with the doctrines of Gall and Spurzheim. In particular, Bell's localisation of consciousness in the cerebral hemispheres accorded with the main dogma of the phrenologists. This notion of the function of the cerebrum received further reinforcement from the experiments of Flourens in the 1820s, which appeared to corroborate the dependence of the will and the other 'higher powers' of the mind upon the integrity of the hemispheres. In addition, Magendie, through a more ruthless use of vivisection than Bell had attempted, in 1822 sustained the view that the nerves which passed through the anterior column of the spine performed a purely motor role, while those which travelled along the posterior

column were exclusively sensory.<sup>7</sup>

In 1836 Bell restated his scheme of the nervous system in greater detail. He took special pains to trace the paths of the different nerve fibres into the brain and thereby to establish the functional relationships between the different parts of the nervous system. The anterior bundles, or 'fasciculi', of nerves could, he argued, be followed as they entered the anterior (motor) column of the spinal marrow; from there

we can trace, without suspicion of error, the anterior column of the spinal marrow upwards into the cerebrum: First, into the anterior **corpus pyramidale**; next through the **pons Variolii**; then forming the anterior part of the **crus cerebri** (anterior to the corpus nigrum) and, finally, into the cerebrum.<sup>8</sup>

The posterior fasciculi could similarly be traced up into

the posterior corpus pyramidale, that which lies in the fourth ventricle, forming with its fellow the corpus scriptorium. It goes then under the **valvula cerebri**, having formed the posterior part of the pons Variolii, enters into the crus cerebri behind the **corpus nigrum**, joins with the anterior **tractus**, but without mixing <sup>with</sup> it; and these together disperse fan-like into the cineritious matter of the cerebrum, the **corpus striatum** and the **thalamus nervi optici** receiving their fibres, and giving them out to the great part of the cerebrum.<sup>9</sup>

In this manner, Bell claimed to have confirmed both the structural - functional duality of the nervous system and to have provided anatomical evidence that it was the cerebrum, in which the sensory fibres ultimately terminated and where the motor fibres originated, that was the seat of consciousness.

But while he revised the received notions of the form of the nervous system in this way, Bell remained conservative respecting its mode of operation. In fact, he merely provided an anatomically more specific account of how the action of the central nervous system depended upon access to the organ of mind and upon the determinations of the conscious principle which he held to reside in 'the cineritious matter, which is chiefly external, and forming the surface of the cerebrum'.<sup>10</sup> The hemispheres possessed processes (the crus cerebri) which received the proximal ends of the sensory and motor columns; the 'sensorium' received the impressions from the former and the will was exerted

through the latter.<sup>11</sup> The only source of active power in Bell's scheme remained, not the entire encephalon, but one part of it: the cerebrum. The rest of the brain — the pyramidal bodies and the remainder of the medulla oblongata, the pons variolii and the crus cerebri — were merely extensions and junctions of the nervous tracts, which, like the nerves, served only to transmit to and from the sensorium — 'without intricacy'.<sup>12</sup>

However, by 1836, even Bell acknowledged that this notion of the working of the nervous system was inadequate.<sup>13</sup> Other neurologists, notably Marshall Hall, had in the course of the 1830s put forward an alternative notion of the action of the spinal nerves; this, when coupled with a redefinition of the anatomy of the cerebro-spinal axis, effected a still more basic shift in British neuroscience than that achieved by Bell. The product of this movement was a concept of the nervous organisation that permitted the formulation of a new theory of the relation of the mind to the body contrary to the conventional one which Bell had maintained.

Marshall Hall's colleagues, much to his annoyance, never tired of reminding him that the concept of reflex action was not his invention. With hindsight, it could be made to seem a natural inference from the venerable distinction between the 'animal' and 'vital' functions. The latter included all the 'internal' functions of circulation, respiration, secretion and excretion; since these went on without the action of the will, it was argued that they must possess some other form of regulation.

The kernel of the notion that this regulation was performed by a mechanism which excited movements in response to stimuli could be discovered in the physiology of Descartes, Whytt and Haller, and had received an especially clear statement at the end of the eighteenth century from Unzer and Prochaska.<sup>14</sup> The latter had in 1784 defined the basic principles of reflexion: namely, that it occurred at a centre somewhere in the neural canal where sensory impressions gave rise to motor impulses; volition and consciousness were incidental to its action — it was a property of the **matter** of the nervous system, not of the spirit; nonetheless, reflexion was 'intelligent' in its operation — it regulated the actions of the animal so as to enhance its chances of survival.<sup>15</sup>

Discussions of unconscious regulation in the early nineteenth century tended to ignore Prochaska's suggestion that 'external' as well as 'internal' functions could be viewed in these terms. More common

was the position expounded by Bichat which made a sharp distinction between animal and vital, or 'organic' functions, and assigned different mechanisms, working on divergent principles, to their control. It was a characteristic of the animal functions that their action depended upon the superintendence of the sensorium commune; on the other hand, the organic functions were under a decentralised and unconscious control.<sup>16</sup> A similar view was expressed by Gall and Spurzheim, who did not detract from the dominance of the cerebrum over animal actions, but confined actions that occurred through organisation alone and without consciousness to the 'organic life'.<sup>17</sup> Marshall Hall's achievement was to revive interest in the notion that it was possible to explain the movements executed by the central nervous system in terms of the operation of an automatic, self-regulating mechanism. He did this in a series of monographs published between 1837 and 1850.<sup>18</sup> Throughout, Hall stressed that he was trying to **supplement**, not to supplant, the existing notion of how the nervous system was governed. There were, he argued, two possible causes of muscular contraction, the 'centric' and the 'eccentric': the one was generated by the cerebrum and caused voluntary motions; the other 'does not originate in any central part of the nervous system, but at a distance from that centre'.<sup>19</sup> In fact, the centre of action was somewhere in the spinal marrow, and its operation was free of the will being

**excited** by the application of appropriate stimuli ..... to certain membraneous parts, whence the impression is carried to the medulla, **reflected**, and reconducted to the part impressed, or conducted to a part remote from it, in which muscular contraction is effected.<sup>20</sup>

Because these processes continued in the absence of the cerebral hemispheres, Hall argued that reflexion must depend on a property of the nervous system independent of consciousness. This property, moreover, was not confined to spine only: it was also present 'in the medulla oblongata independently of the medulla spinalis; in the spinal marrow of the anterior extremities', as well as of the posterior extremities of the neural canal.<sup>21</sup>

In his later work, Hall struggled to clarify his notion of reflex action, in the face of widespread hostility and misunderstanding.<sup>22</sup> He was especially anxious to convince others that reflexion did not challenge the role which had been normally assigned to the cerebrum, the organ of mind, but merely accounted for a narrowly-defined group of actions which were unworthy of direct cerebral government.



In order to underline the absolute distinction that existed between voluntary and reflex actions, Hall postulated an entirely separate nervous apparatus for each: there was a 'True Spinal Marrow', and a system of 'excito-motory' nerves that served the various reflexive centres, and there were also 'sensory and voluntary nerves, which proceed to and from the **cerebrum** as their centre'.<sup>23</sup>

Hall conceded that there were no **anatomical** grounds for this distinction, but argued that the **physiological** division of labour he put forward required the assumption of a corresponding structural differentiation. The functions peculiar to the cerebro-spinal system included perception and voluntary movement; in consequence, Hall declared the sovereignty of the spirit was not threatened by his scheme. In the cerebrum the psyche sat enthroned: 'the voluntary nerves convey the mandates of volition to the muscles which are to be called into action. All these functions are strictly **psychical**. They imply consciousness'.<sup>24</sup> The 'true spinal' system, while independent of volition and consciousness, was confined to the regulation of the processes of '**Ingestion, Retention, Egestion, and Exclusion**, and, above all, in the vital functions of **Respiration**'.<sup>25</sup>

Having thus secured himself against charges of infidelity, Hall proceeded to apply his concept of reflexion to an understanding of certain diseases of the spinal cord. At the same time, he continued to refine his terminology, until by 1850 he had arrived at an explicit statement of the concept of a 'reflex arc': each of these arcs comprised an 'esodic' nerve, and 'exodic' nerve and a 'spinal centre'.<sup>26</sup> The former two presumably corresponded to Bell's sensory and motor nerves (although Hall denied that his ideas had been inspired by those categories); however, the anatomical locus of the 'nervous centre' remained vague — it was situated somewhere in the spinal marrow, but Hall never attempted to specify its character.

Even before Hall's work became widely known and accepted, however, other British physiologists had already begun to seek the histological correlates of 'nervous power'. Indeed, this search was the main form of scientific practice arising from the study of the nervous system during this period. To some extent the British scene resembled that in Europe in this respect: there too the gross and minute anatomy of the brain and nerves were subjects of intense investigation.

But in France and Germany such enquiries were matched by the ablation experiments of **Flourens**

and the experimental studies of the physicalist school. In Britain these more strictly

**physiological** approaches to the nervous system were lacking for a number of reasons. Hostility to

vivisection was one important contributant. More generally, however, neurology reflected the 'anatomical' bias of British life science in general; a bias which, as discussed in the preceding chapter, was the result of the atomistic and underfunded nature of biology in Britain in the first half of the nineteenth century.

Because of the context in which it operated, British neurology in the 1840s concentrated upon achieving a morphology of the essential elements of the nervous system. In particular, by 1850 a structural unit for the centre of reflexion had been defined; in the process a new way of conceiving the form and function of the nervous system was devised.

While the old 'doctrine of the anatomical schools', which Bell had described and only partly revised, had held that the brain was the unique source of power in the nervous system, the chief feature of the scheme that replaced it was that power was distributed among a number of centres. The contrast between active and commissural parts was maintained and a histological basis for it provided; but the cerebrum was held to be only one of several homologous structures which performed the former office.

This redefinition relied on two moves; The first was the extension of the concept of the 'ganglion', already in use in the explication of the 'sympathetic' nervous system, to the cerebro-spinal. The second was the institution of criteria for what it was to be a 'ganglion', which enabled this concept to be extended to any part of the nervous system that contained grey matter, including the hemispheres themselves. The chief interest that stimulated this renegotiation of terms lay with those anatomists in Britain and Europe, who espoused an essentially **comparative** approach to the anatomy of the human nervous system.

Their case was that neural anatomy, as presently constituted, lacked any central idea about which observations could be organised. Such a nodal concept was, however, available in the method of comparative study developed in France by Cuvier and, more especially, by Serres. The latter had taken from Geoffroy St. Hilaire the theory that all actual animal forms were variants upon an ideal 'type', which embodied their most essential features. In 1824 Serres applied this principle to the brain, and had concluded that '*L'encephale des animaux vertébrés est donc construit sur un type uniforme et avec les mêmes éléments*'. He also argued that the **progressive** development of this archetype was evident

in the **scala natura**: organs which were at first simple, gradually grew more complex, without ever losing their basic identity. In consequence, it was possible to trace a succession of stages that connected the lowest vertebrate with man.<sup>27</sup>

The study of the human nervous system could, on this principle, be organised around the discrimination of the homologues that existed between this and lower forms of neural organisation. In Britain, Samuel Solly argued the value of this approach; he claimed that

the only philosophical method of simplifying and giving a character of general interest to the anatomy of the human brain, is by commencing with the structure and function of a nervous system in the lowest and simplest forms of animal existence, and from this rising by degrees to the highest, carefully observing each addition of parts, and the relationship borne by these to an addition of function.

Solly stressed one particularly important result of following this method — the discovery

that the encephalon, this apparently most complicated organ in the human being, is but a gradual development from an extremely simple fundamental type on one uniform and harmonious plan.<sup>28</sup>

But in order to show that the encephalon was, indeed, the **culmination** of the spinal column, rather than an alien organ that happened to be on top of it, some means were needed to mediate the apparently vast gap between the human nervous system and those of animals which either lacked cerebral hemispheres, or which got along with very rudimentary cerebral development. A similar gap appeared on the functional side: if the cerebrum were the exclusive seat of nervous power, then the human nervous system appeared to operate on entirely different lines from those of the lower animals in whom nervous direction was widely dispersed. The strategy which Solly adopted to meet these problems was to assert that all nervous systems contained three basic elements, and to see all further neural structures as developments of these.

The 'fundamental type' of the nervous system was, Solly argued, found in the Echinoderms: it comprised 'ganglia, commissures and nerves'; 'ganglia' formed the central unit to which the sensory and motor nerves were connected. This typology depended upon the extension by analogy to the central nervous system of ideas of structure and function that had been developed in relation to the sympathetic

(or 'vital' or 'organic') nervous system.

Cuvier had assigned the regulation of the 'vegetable' functions to an independent 'ganglionic' nervous system, holding this conception to be preferable to the immaterial vital principle which Stahl's school had charged with the duty.<sup>29</sup> Bichat had enlarged upon the same theme in his *Recherches physiologiques sur la vie et la mort*. There he had held that the regulation of speech, movement and the other 'animal' functions was unproblematic: their organs ceased to operate when their connection with the brain was severed; it followed that they were cerebrally controlled. The 'grande difficulté' related to organic functions which seemed to subsist independently of the encephalon; moreover, anatomically, the organs which performed these offices 'ne reçoivent point ou presque point de nerfs cérébraux, mais bien des filets provenant des ganglions'.<sup>30</sup> Bichat had concluded, therefore, that the 'ganglionic nervous system' was no more than 'un ensemble d'autant de petits systèmes nerveux qu'il y a de ganglions, lesquels sont des centres particuliers de la vie organique, analogues au grand et unique centre nerveux de la vie animal, qui est le cerveau'.<sup>31</sup>

Subsequently, Bichat continued to stress that the sympathetic nervous system was composed of a number of autonomous centres of nervous power, or ganglia. These structures he defined as 'petits corps rougeâtres ou grisâtres, situés en différentes parties du corps, et formant comme autant de centres d'où partent une infinité des ramifications nerveuses'. This ganglionic system was independent of the brain in its histological 'texture', as well as anatomically and physiologically.<sup>32</sup>

Bichat's conception of the sympathetic nervous system afforded, later in the century, a resource for those who wished to reform existing notions to make them more amenable to the ends of comparative anatomy. The 'ganglion' could provide the basic unit of neural organisation which would unite all animal forms, if the reference of the concept to the nervous system as a whole could be established. This Bichat and his followers had denied; they had attempted to create an absolute distinction between the sympathetic and the central nervous systems.<sup>33</sup> However, in the course of the 1830s and 1840s these barriers were broken down, and the way opened to a view of the human nervous system as a series of structurally and functionally homologous ganglia.

Bell had reviewed Bichat's classification of the nervous system in 1836, and had objected that the rigid division between the ganglionic and the cerebral nervous systems must collapse because, even by

Bichat's own criteria, there were ganglionic structures in the central as well as in the sympathetic organisation.<sup>34</sup> Solly went further: he recognised that the utility of Bichat's concepts to his concerns was vitiated by restricting ganglia to small bundles of grey matter. He argued that such criteria of size and shape were irrelevant, and that the continuance of the narrow concept of the ganglion was harmful to the development of neuro-science; in particular, as a consequence of such false definitions of ganglia,

the medical student has been led to imagine that the neurine which is contained within the human skull is altogether different from the ganglia of the lower orders, merely because it differs from them so much in its outward appearance.<sup>35</sup>

The proper criterion by which to identify ganglia, Solly declared, was histological composition, rather than gross structure. He argued that the term 'ganglion' should be applied 'to any collection of cineritious neurine into a circumscribed mass, whatever form of arrangement it may assume'. By this standard,

the cineritious neurine which forms the convoluted surface of the hemispheres of the human brain I should denominate the hemispherical ganglia.<sup>36</sup>

More generally, the brain and spinal column in their entirety were to be understood as 'a series of large ganglia', comparable to the chains clearly evident in the Arthropods.<sup>37</sup>

Solly's early ideas as to what 'grey matter' was were vague. But microscopical studies in the 1840s provided a more exact notion of what constituted the 'active' component of the nervous system. This work also assisted the assimilation of the working of the nervous system to a growing body of contemporary thought about the histological basis of all bodily functions outlined in Chapter Three.

By 1847, when the second edition of his textbook appeared, Solly had incorporated these resources into his account. The cineritious neurine was distinguished by its 'vesicular' nature: it consisted 'almost entirely of cells with nuclei and nucleoli'.<sup>38</sup> Ehrenberg in 1833 had detected the presence of peculiar cells in the grey matter of the nervous system, and his work had encouraged further studies. Johannes Müller reported these researches in his physiological manual and concluded that the 'grey substance of the brain and spinal cord is ..... formed wholly of the same globules as the

ganglia of vertebrated animals'.<sup>39</sup> These findings were confirmed and amplified after 1840 by a group of workers at London University, notably Robert Todd, who further specified the distinctive morphology of the brain cell.<sup>40</sup>

Solly followed Todd in identifying these vesicles as the seat of nervous power — the 'grey matter' being merely a conglomeration of such centres; in this he could claim support from the efforts of workers in other fields to use the cell as a link between anatomical and physiological concepts. In his **Anatomical and pathological observations** of 1845, John Goodsir had remarked that the labours of Schleiden and Schwann had not only revealed cells to be 'the germs of all tissues', but also 'the immediate agents of secretion'. The basic agent in all bodily processes was, therefore, 'the primary cell endowed with a peculiar organic agency, according to the secretion it is destined to produce'.<sup>41</sup> By analogy, Solly argued, it was 'most probable that the nucleated cells of vesicular neurine are the active agents in the production of nervous power'; like the 'acknowledged secreting organs', the vesicular structures of the nervous system were engaged in producing 'a something', that made the nervous function occur.<sup>42</sup>

What this 'something' was remained a matter of controversy throughout the nineteenth century. However, the close affinities that were found between its mechanism and that of more mundane functions, made it easy to argue that the nerve force was a merely physical entity. Moreover, while the nature of nervous power remained mysterious, the manner in which it operated was open to interpretation in terms of reflex action, and this strengthened the trend towards a naturalistic model of the nervous system.

Disagreement persisted in the 1840s and 1850s as to how much of the action of cerebro-spinal axis could be referred to reflexion; there was a considerable resistance from those concerned to preserve a physiological role for the mind to a complete mechanisation of nervous function. However, Solly had recited Gall and Spurzheim's dictum that function followed structure, and, in the event, the supposed histological homogeneity of the central nervous system, which extended to the hemispheres themselves, proved a powerful argument for viewing all its actions in terms of reflex action.

By the late 1840s there was general agreement among British physiologists that Marshall Hall had adduced a valid notion of how one class of action was performed in the absence of consciousness

or of cerebral direction. Further, an anatomical analogue for the centres of these excito-motor actions had been found in the 'ganglia' of the spine. Each of these comprised a central mass of grey neurine in communication with sensory and motor – or, to use the terminology that came into favour in these years, the afferent and efferent – nerves.

It had also been agreed that, by this standard, ganglia were not confined to the spine. On the contrary, ganglionic vesicles were to be found in the olivary bodies of the medulla oblongata, in the pons Variolii, in the locus niger of the crus cerebri, in the 'cerebral ganglia' – ie. the optic thalami and the corpora striata – and in the cortex itself.<sup>43</sup> But there was much equivocation and contention as to whether reflexion was applicable to the functions of these 'higher ganglia'.

Todd continued to reserve a separate role for the immaterial mind in the causation of some actions which were not, therefore, simply reflexive.<sup>44</sup> A similar doctrine appeared in the textbook which he produced with Bowman, the same work in which the ubiquity of ganglionic centres in the nervous system received its most detailed expression. Todd and Bowman distinguished two forms of causation, corresponding to two kinds of nervous event: while excito-motor actions were the result of purely physical changes in the nerves and in the ganglia, the will could act upon the nervous system to produce some actions. The latter actions were 'so distinct from anything which observation teaches us to be produced by material agency, that we are bound to refer them to a cause different from that to which we refer the phenomena of living bodies'. This qualification had theological roots: it was prudent for physiologists in the 1840s to preserve an active role for the 'immortal soul of man'.<sup>45</sup>

However, Todd and Bowman also provided resources for ending this dualism and for matching the anatomical (ganglionic) unity of the nervous system with a physiological (reflexive) continuity. For instance, they denied that the 'voluntary' acts took place through a separate apparatus from the sensory-motor: Hall's theory of two sets of nerves had no foundation.<sup>46</sup> In consequence, an action which might at one time seem voluntary could, at another, be excito-motor; the difference between the two was psychological not physiological.<sup>47</sup> Moreover, Todd and Bowman carried the description of the functions of the higher ganglia in terms of reflexion as far as the optic thalami and the corpora striata. These two structures were held to deal with each of the aspects of ganglionic

function: the optic thalami being afferent centres, the corpora striata efferent. The homologues of the sensory and motor nerves which served the lower ganglia were detected in the anterior and olivary columns of the medulla oblongata.<sup>48</sup>

As Todd and Bowman had stressed, spinal reflex actions were 'purely physical in their cause' and 'independent of all mental influence'.<sup>49</sup> If the higher centres were composed of the same materials and continuous with the cord, it was at least possible that they too were regulated by physical agency alone. If this was so, what was the physiological role of consciousness?

Todd and Bowman's writings suggest these questions because the potential in their system was seized upon to expand the reference of reflexion to all parts of the nervous system and to establish a fundamentally different conception of the relation of the categories of consciousness to bodily events from that enshrined in a dualist interactionism. It is important, however, to avoid the impression of inevitability that hindsight encourages. These developments were the result of the work of particular individuals, who were only in partial agreement with one another, and who pursued different ends by somewhat similar means. Two major clusters of interests in the British biological and medical community between 1840 and 1859 were of particular relevance; these may be designated the psychopathological and the zoological, and examined through the work of Thomas Laycock and W.B. Carpenter respectively.

Carpenter developed the research project which Serres and Solly had adumbrated; he pursued a comparative approach to the study of human anatomy, and encouraged the growth of zoology as an independent branch of biological knowledge in Britain. In the case of the nervous system, the goals of comparative anatomy demanded a concept of neural structure and function which was sufficiently simple, yet inclusive, to encompass all organisms from those with the most basic neural organisation to those with the most complex. Secondly, a comparative anatomy demanded continuity between the different forms of nervous system: the changes between one element in the series and the next should be developments of some feature present in, or at least intimated by, the preceding term. Further, despite change, shared attributes between members of the series should be shown to persist.<sup>50</sup>

The concept of reflexion supplied a physiological idea sufficiently basic to satisfy the first of these requirements. The most essential, primitive and pervasive functions of the nervous system,



Carpenter argued,

take place independently of all consciousness, — by the simple **reflexion** of an impression conveyed to a ganglionic centre by one set of fibres proceeding toward it from the circumference, along another set which pass **from** it to the muscles, and calls them into operation. This reflex function, therefore, is the simplest application of the Nervous System in the animal body.<sup>51</sup>

The ganglionic centres became enormously elaborated in the higher animal, and a concurrent complication occurred in the kind of nervous activity they manifested. In addition to the simple reflex, or excito-motor act, a class of action developed which was accompanied by sensation. Such 'consensual' actions continued, however, to satisfy the physiological conditions of reflexion: they were motor responses to afferent stimuli which differed from excito-motor acts only because the latter did not impinge upon consciousness as sensations. This continuity was affirmed by the achievement of consensual, as of excito-motor actions, through ganglia sharing the same fundamental characteristics.<sup>52</sup>

Carpenter's conception of the central nervous system as a chain of functionally semi-autonomous centres was essentially that of Todd and Bowman applied to the concerns of comparative anatomy. The vertebrate spinal column was the homologue of the ventral chain of the Articulata; within the vertebrate series a progression was visible wherein the ganglia of the encephalon gradually became larger, and within that development, the cerebral hemispheres expanded to cover, more or less completely, the rest of the brain in the higher vertebrates.<sup>53</sup> But although he described the hemispheres as 'ganglia', and viewed them as complications of the other nervous centres of the head, Carpenter was cautious in 1846 about attributing to the cerebrum the reflex function that he held to be common to other ganglia. He implied that at a certain stage in cerebral development a discontinuity had occurred in the working of the nervous system: together with the 'increase of Intelligence' had come 'the predominance of the Will over the involuntary impulses'. The cerebrum was the instrument of the will, and not subject to reflexion. Referring to the failure of Flourens and others to excite the cerebrum artificially, Carpenter concluded 'that the changes which **mental** operations produce in the cerebral fibres cannot be imitated, as changes in other motor fibres may be, by physical impressions'.<sup>54</sup>

Carpenter reviewed this opinion between 1846 and 1853, when the fourth edition of his

**Principles of Human Physiology** appeared, especially in the light of Laycock's work on cerebral function; the alterations that he incorporated in his section on the cerebrum would, Carpenter hoped, open up an entirely new line of enquiry.<sup>55</sup> The principal change to which he referred was the postulation of a third class of reflex action, analogous to excito-motor and consensual acts in that it was occasioned by afferent stimuli being reflected into efferent impulses through a ganglion, but distinct in being accompanied by 'intellectual' states. The ganglion involved in these actions was the cerebrum, and Carpenter maintained that the resulting motions must be considered as manifestations of the 'reflex power of the Cerebrum, and consequently as no less automatic in their character than those which result from the reflex power of the Cranio-Spinal axis'. Carpenter named such actions 'ideo-motor', when they were accompanied by ideas, and 'emotional' when by a feeling.<sup>56</sup>

Carpenter stipulated that ideo-motor reflexes only occurred when the will was, for some reason, in abeyance and unable to discharge its normal control over the sensations received and the instructions issued by the hemispheres. He was not concerned to undermine the position of a causally-effective spiritual principle; on the contrary, he upheld it. What Carpenter did attempt was to link the activity of the cerebrum with that of the other ganglia; thereby, the notion of a gradual, correlated development of structure and function in the animal nervous system could be maintained. By drawing the hemispheres at least partially into the framework of reflexion, Carpenter had narrowed the great gap that separated them from the rest of the neural organisation.

However the position of the cerebrum within the human nervous system posed a more fundamental problem to the serial continuity that comparative anatomy required. The cerebrum was non-existent, or rudimentary, in much of the animal kingdom; nonetheless, acephalous or neo-acephalous creatures possessed efficient nervous systems. The hemispheres appeared, therefore, to be functionally superfluous, and this impression was heightened by their 'superimposed' anatomical character: they seemed to be late additions grafted onto an already complete system, comprising a dorsal chain of ganglia, the medulla oblongata and a series of 'sensory' ganglia.<sup>57</sup>

If the human nervous system were, indeed, homologous with that of the lower animals, then the spine, medulla oblongata, the optic thalami and other sensory ganglia comprised a self-sufficient unit, capable of coordinating the most complex actions through reflexion. The cerebrum was superfluous.

However, the hemispheres were, by common consent, 'the instrument of those psychical operations, which are superadded, in Man and the higher Vertebrata, to mere sensations' — namely ideas and emotions, the main components of personality.<sup>58</sup>

The considerations that comparative anatomy provoked invited the question of what the relation of states of consciousness to nervous actions was. Carpenter himself continued to maintain an active role for the immaterial principle, and thereby to qualify his incorporation of man into the natural order. But for others, with an interest in denying the ability of a non-physical entity to influence the body, comparative anatomy provided one resource for promoting an entirely naturalistic view of the causation of nervous action.

Thomas Laycock approached the problems of brain function with the perspective of a practising physician with a special interest in nervous diseases, such as hysteria. He held that these diseases were disorders of the emotions as well as of the motor processes, and had to be regarded as the results of dysfunctions of the nervous system. In particular, he urged the medical profession to examine the value of Hall's ideas of reflex action to understanding of these neuroses, not in respect 'of the spinal cord only and its prolongations, but to the brain also'.<sup>59</sup>

Laycock stressed 'analogy' as a valid form of physiological inference, and justified his extrapolations from the lower to the higher forms of nervous action by reference to 'the unity of nature', as exemplified in the homologies between the forms and functions of different animals.<sup>60</sup> According to this principle, the character of the higher was immanent in that of the lower; consequently,

the cranial ganglia, although the organ of consciousness, are subject to the same laws as those which govern the other ganglia ..... In short, ..... the passions and the movements dependent on them .....; the instinctive feelings and their conjoined movements; and the whole series of convulsive movements, from those of epilepsy and chorea down to the retraction of the foot by a decapitated frog, are seated in the same grand division of the nervous system [ie. into afferent and efferent fibres], and depend on the same general laws.<sup>61</sup>

Laycock amplified this view in 1844, when he posited a gradual transition between the different functions of the nervous system, corresponding to the progressive modification of its structure: 'The automatic acts pass insensibly into the reflex, the reflex acts into the instinctive, the instinctive are

quasi emotional, the emotional are intellectual'.<sup>62</sup>

Because his concern was **mental**, as well as merely 'nervous' disorders, Laycock was forced to confront the metaphysical issues of the relation of mind and body directly. Specifically, he undertook to revise the conventional wisdom respecting the relation of consciousness to organisation to which Hall, Solly, Todd and Bowman and Carpenter had deferred. His doctrines would, he wrote George Combe in 1845, 'modify very much indeed many of our present metaphysical doctrines and place spiritualism on a much lesser basis than it at present possesses'.<sup>63</sup>

Laycock attacked such 'spiritualism' as inimical to his somatic pathology of mental (especially emotional) disturbance. His experience at York County Hospital in the 1840s had, he wrote, convinced him that morbid conditions of the nervous system had deleterious effects on the personality. However, when he tried to formalise these conclusions, he found no help from existing metaphysical theories; these held the cerebrum to be the instrument of an autonomous spiritual entity, and the cause of mental derangement to lie, not in the nervous mechanism, but in this soul. Such notions were useless 'to explain or elucidate functional diseases of the brain'.<sup>64</sup>

A psychology that would be serviceable to the aims of a physical pathology of mind needed two elements: a theory of the **dependence** (not merely the connection) of mental states upon bodily organisation, and another which eliminated consciousness as an independent cause of nervous action. The first of these Laycock achieved by combining the resources of phrenology with those of a materialist theological doctrine which went back, via Joseph Priestley, to the sectaries of the seventeenth century.

Laycock's family background lay in a provincial, dissenting Christian tradition, which maintained the dogma of bodily resurrection, as opposed to the belief in the survival of an immaterial essence; the latter view had the sanction of the Established Church. A corollary of the doctrine of the resurrection of the dead was the denial that mind and matter were entirely separate entities: Laycock argued, for instance, that the divine mind could not be conceived as absolutely other than its creation. Moreover, he held that the human 'mind is a result of organization', and, while still a medical student, was attracted by the theology of Andrew Carmichael, which afforded Biblical authority for the notion 'that man's soul has not an independent existence'.<sup>65</sup>

Late in the 1830s, when he was commencing his psycho-physiological researches, Laycock drew

confirmation for these views from the philosophical school of Leibniz. More particularly, as Laycock's interests focussed upon the neurotic correlates of psychotic disturbance, he looked to the writings of phrenologists to supply a conception of the relation of mental faculties to bodily structures. Laycock never became a self-proclaimed phrenologist; rather he took from the creed its fundamental tenets and jettisoned its more peculiar notions.<sup>66</sup> He informed George Combe in 1845 that

I acquiesce generally too in the principle of phrenology — that the brain is the organ of mind— ..... [and] that the different propensities and faculties have corresponding cerebral scites.

But, in the same letter, Laycock queried the particular sites the phrenologists had found for certain faculties.<sup>67</sup>

Similarly, when in 1859 Laycock provided the article on 'Phrenology' for the new edition of the *Encyclopaedia Britannica*, he concurred with the general principles of the science — namely, 'that the mind and body are inseparable in this world' and that the 'brain is the immediate organ of the mind'. To phrenology, Laycock continued

may be fairly conceded the grand merit of having forced the inductive method of inquiry into mental philosophy, and thus laid the permanent foundations of true mental science.<sup>68</sup>

However, he dissociated himself from the more specific phrenological practice of cranioscopy.

Laycock looked beyond phrenology to the new cerebral histology to supply a closer conception of the anatomical seat of the mental functions. As well as being a ganglion, a centre of reflex action, possessing 'kinetic' power, the cerebrum was also the centre of consciousness: its cells had an 'ideagenic' potency.<sup>69</sup> Laycock's pathological preferences inclined him to deny that the former 'kinetic' properties of the cerebrum were caused, or influenced by its 'ideagenic' propensity, but to maintain that the manifestation of both depended on the healthy action of their material substratum. Consciousness was 'coincident' with the acts mediated by the cerebrum, but 'independent of it'. This proposition, Laycock told Combe,

is essential to my system. It is this which will enable us to apply the laws of spinal action to the explanation of cerebral action and (of course) of mental philosophy and all practical questions connected with it.<sup>70</sup>

In his efforts to establish a purely physical causation for motor and emotional disorders, Laycock therefore arrived at a view of states of consciousness as incidental to the reflex actions of the cortex, but nonetheless determined by them. He later extended this principle from emotional to 'volitional' acts, arguing that

although, as the organ of conscious mind, [the cerebral] ..... functions were carried on **with** consciousness, yet as a series of ganglia analogous to the spinal, its functions might be, and often were, carried on **without**, or at least independently of the will, and of the accompanying sensations, if consciousness existed.<sup>71</sup>

The full implications of this view were hardly realised at first, but they were to be of great future significance; as one later commentator put it:

If ..... mental activities are capable of being excited and enacted involuntarily and unknowingly as a product of cerebral life, the intermediacy of "the mind" becomes obviously unnecessary; and if the most recondite processes of consciousness can be carried on unconsciously, consciousness is clearly not co-extensive with mental activity, and is only a partial and incomplete evidence of the operations of "the mind".<sup>72</sup>

Some of the consequences for neurology and psychology that followed from a working-out of these ideas are discussed below. But it is necessary to stress, as Laycock himself did, that in the 1850s: he stood 'almost alone in maintaining that in the so-called **sensational** actions, sensation or consciousness takes no share causally, and is only a coincident phenomena not necessary to the actions'.<sup>73</sup> Others, even when they were sympathetic to the application of physiological notions to mental science, were less decisive in excluding consciousness as a causal agent in the nervous system.

Alexander Bain who, together with Herbert Spencer, was viewed as the doyen of physiological psychology, exemplifies this ambiguous attitude to the role of consciousness. On the one hand, Bain stressed that the germs of 'volitional' action were evident in the nervous system prior to the appearance of consciousness; it followed that 'the ultimate source of voluntary motion cannot depend on any conscious conception of its object'. Instead, the feeling of volition arose as a parallel development to the exercise of the potential for purposeful action inherent in the nervous matter.<sup>74</sup> But, on the other hand, Bain vitiated the view that states of consciousness **paralleled** nervous changes without influencing

them, by maintaining that 'as soon as a clear consciousness of movements sensibly remedial comes into play, ..... consciousness has a power of stimulating a concurring action; in other words volition has begun'.<sup>75</sup>

Such uncertainty about the extent of the dependence of the mind on the nervous system and of the relevance or irrelevance of consciousness to the action of the latter shows that, despite the claims of their proponents, 'strong' somatic theories of mind were not necessary inferences from the 'facts' of anatomy and physiology. Rather they were interpretations that were actively imposed upon the materials at hand, which were reshaped to fit various preconceptions. The deployment of physicalist mental philosophies needs to be referred, therefore, to particular interests in Britain between 1850 and 1880, and especially to be related to the development of the scientific and medical professions during this period.

## ii. A Mental Pathology

Je dus, en conséquence, conclure que les facultés des hommes et des animaux sont inées.

Mais alors il se presentoit cette question: sur quoi est fondé cet état des choses? Est-ce un principe particulier dont les facultés elles-mêmes constituent l'essence, et qui soit en même temps doué du libre exercice de ces facultés; ou bien ce principe, et l'exercice de ses facultés, sont ils subordonnés à certaines conditions materielles?

Si ce principe jouit de l'exercice de ses facultés independamment de l'organisation, il est, ainsi que tous les fonctions, hors de la sphere du physiologiste; le metaphysician et le theologien peuvent seule prononcer sur sa nature.<sup>76</sup>

(Franz Josef Gall)

What, then, you ask, is meant by this legal term, unsoundness of mind? in what way does it become a branch of medical study and practice? by what application of the art and science of medicine is it to be removed?

The answer is, that unsoundness of mind is but another term for disorder of the human brain, or rather of that portion of the nervous matter which has for its function that which we call mind and mental operations.<sup>77</sup>

(G. Fielding Blanford)

The physiological and anatomical doctrines discussed above were developed almost exclusively by medical men and directed towards the supposed pedagogical and therapeutic needs of their profession. One section of the medical community was especially receptive to these ideas and active in developing and expounding them: they were the psychiatrists, or, as they were then more commonly known, the 'alienists' or 'mad doctors'. These were chiefly based in the state-owned County Asylums by the latter half of the nineteenth century, and had by 1841 achieved a sufficient sense of corporate identity to band together into the Association of Medical Officers of Asylums and Hospitals of the Insane, and to produce a professional periodical — the *Asylum Journal*, later the *Journal of Mental Science*. There is a striking correspondence between members of the Association and the chief expounders of somatic theories of mind in the period from about 1850 to 1880:<sup>78</sup> in particular, British alienists exerted themselves in these years to derive a physicalist pathology of mind from the notions of the



cerebrum as the seat of mentality and of reflexion as the mode of its functioning.

In 1844, Beverley Morris, physician to the York Dispensary, provided a stimulus to such efforts by extending Laycock's analogy between the convulsive diseases caused by spinal dysfunctions and the 'emotional convulsions' of hysteria to the whole range of mental illness. Proceeding from the homologous structure and functions of the brain and spinal cord, Morris concluded that 'in irritation of the brain we may have two sets of effects produced — one manifested by some impairment of its mental faculties, and the other by the induction of some distant disease'. Just as irritation of the spinal ganglia impaired their motor functions, so 'when irritation exists in the brain ..... its mental functions are disordered'.<sup>79</sup>

Morris did not regard such physical causes as the **only** source of madness: he also allowed a role for 'spiritual' causation. But the tendency of subsequent writers was to retain and to elaborate the material causes of insanity, while excluding those derived from the soul. Henry Monro discussed the various views of the causes of madness in 1851. One theory saw insanity as 'wholly independent of physical considerations' and caused by 'spiritual agency of an evil nature'; secondly, the body might be allowed an instrumental part, as when the 'evil spiritual agency' manifested itself by impairing the normal functions of the body; lastly, there were purely physical causes. Monro declared his conviction that **'insanity is essentially dependent on this last source of mental obliquity'**.<sup>80</sup>

In its healthy state, Monro argued, the cerebrum enjoyed an equilibrium between afferent and efferent impulses and performed all its functions, physical and mental, efficiently. However, when this 'nerve tone' was lost, and the equilibrium disturbed either by an excess or by a deficit of stimulation, pathological bodily and psychic states resulted.<sup>81</sup>

Just as the sound reflex function of the brain had been referred to an histological foundation in the 1840s, so were cerebral dysfunctions explained in terms of structural deformities in the tissue of the hemispheres. The authors of the major textbook of medical psychology in mid-Victorian Britain made it a leading principle of their science that pathological and physiological principles should be intertwined in this way: a 'rational pathology' was 'an account of the abnormalities of organization and of function, which as much depends on the nature of the laws of life as our health'. While mental health depended upon 'the repose of the brain', cerebral pathology rested on the principle that 'mental

diseases result from the interruption or disruption' of the condition of the healthy function of the grey matter of the cortex.<sup>82</sup>

Attention was focussed upon the vesicular neurine of the cerebrum in these investigations. The brain cells were the source of the nervous force of the brain; moreover, they were 'the agents of all that is called mind .....; and the growth and renovation of these cells are the most ultimate condition of mind with which we are acquainted'.<sup>83</sup> Those who tried to locate the seat of mental illness in identifiable morbid alterations of these cells and of the structures that supplied them with nourishment gained support from the general trend in pathology at this time. Adam Addison wrote in 1861 that the recent revolution in medical science was specifically dependent upon the search for 'certain gross, palpable changes of structure having an invariable causal connection with functional disturbances'. Virchow had, moreover, located the cell as the ultimate seat of disease; it was merely an extrapolation from this model of pathology to look to the structure of the neural vesicles for an explanation of mental disorder.<sup>84</sup>

Special stress was placed upon the importance of proper circulation of the blood in the cerebrum to the maintenance of the brain cells in a healthy state. This involved a certain amount of backtracking by mental pathologists from their initial confident assertion that the cell itself was the real object of study. Bucknill and Tuke, for instance, argued that the way in which the form of the cell contrived to produce nervous and mental activity was beyond discovery, and that vesicular structure was too minute for its morbid changes to be identified; on the other hand, it could be confidently stated that the operations of the brain cells (whatever unknown processes these might involve) were clearly dependent on the state of the fine network of capillary vessels which permeated the cerebral neurine, and pathogenic changes in this circulatory system were discernible.<sup>85</sup> Similarly, Forbes Winslow maintained that post-mortem examination of the brains of the insane frequently revealed 'irritation, capillary congestion, inflammation, ..... toxic agents circulating in the blood producing modifications of cerebral nutrition, morbid changes in the coats of the blood vessels'. To such changes, he held, could be attributed the insanity of their sufferers, capillary congestion being the most common instigator of mental disease.<sup>86</sup> There was much disagreement among pathologists about the precise causation of particular kinds of insanity; but this took place within the framework of agreement on the general

principle that there was 'a causal relationship between congestion [of the cerebral blood vessels] and insanity'.<sup>87</sup>

The reasons for the interest and support given by British alienists to such somatic theories of mental illness might be looked for in the instrumental value of these notions in the treatment of patients. Certainly, Bucknill and Tuke declared it to be a major aim of therapy 'To remove the pathological condition of the brain', which was the cause of insanity.<sup>88</sup> However, the therapeutic techniques they went on to enumerate showed little connection with the pathological theories they had expounded. One practical recommendation which could be linked to their cerebral pathology was to use bleeding as a means of relieving the hyperaemia of the hemispheres caused by blocked blood vessels. In addition, because congestion in the head was supposed to depend upon general congestion of the bodily systems, purgatives were prescribed to relieve both. Further, rubbing 'counter-irritants' into the scalp would, it was held, draw to the surface whatever was irritating the cortex and thus ease cerebral inflammation.<sup>89</sup>

But even these 'remedies' were long-established parts of the armoury of the alienist, and had been employed upon the insane for over a century independently of the elaborate histological pathology with which they were coupled in the 1850s and 1860s. Bleeding had been one of the panaceas of eighteenth century medicine and was lavishly used in the asylums; purgation was as venerable — the chief difference between its use in the mid-eighteenth and in the mid-nineteenth century lay in the substances that were administered.<sup>90</sup> Bucknill and Tuke were somewhat embarrassed by the crudity of these methods, but they clearly had little else to offer to the practising alienist.

The other treatments they mentioned were as traditional and had still less to do with their pathological doctrines. In fact, the main rationale of most of their therapy was to keep the patient quiet rather than to effect a 'cure'. Opium, cold showers, warm baths and wet blankets were all recommended for this purpose.<sup>91</sup> The likely debilitating effects of continual bleeding and purging can also be accommodated to the view that the aim of alienists was to make their patients more amenable to control, and that somatic theories of mind had little impact upon their clinical practice. Although, in 1867, Henry Maudsley declared that a 'truly scientific treatment will be grounded upon the removal of those bodily conditions which appear to have acted as causes of [mental] disease', the treatment he suggested

also fell into the above pattern of suppressing symptoms rather than remedying causes.<sup>92</sup>

If the instrumental value of somatic theories of mind was so slight some other use for these conceptions to British alienists needs to be found. One function which they evidently played in the life of the psychiatric profession in the mid-nineteenth century was to provide a token of group membership, and to serve as an expression of solidarity of interest among mad doctors. On two occasions W.A.F. Browne, Superintendent of the Crichton Institute at Dunbar and a prominent member of the Asylum Officers Association, wrote to request George Combe's assistance in securing the appointment of a particular candidate on the grounds that 'He holds correct views on the nature of Insanity', and 'It appears to me important that a man of practical experience in the management of the Insane in seclusion — and holding correct views of the nature of Insanity should be appointed'.<sup>93</sup> The views referred to were those which regarded 'Insanity as a disease of the afferent parts of the tissues of which the mental phenomena are ..... the symptoms'.<sup>94</sup>

Such talk of 'correct views' is reminiscent of the idiom of political parties organised around an explicit ideology. The function of doctrine in such groups is two-fold: it helps secure the internal cohesion of the group and to define the party against other sects. Browne confirmed this view of an essentially political function for physicalist theories of mental disease when he held that deviation from these principles by physicians

cannot be regarded in any better light than as a treason to the principles of our profession.<sup>95</sup>

Somatic pathologies of mind had acquired this kind of political significance early in the nineteenth century. Late in the previous century physicians had moved to establish a hold over the new public asylums and to exclude laymen from the superintendence of these institutions. Their exclusive control was challenged, however, by other interests who also competed for a share of the power and jobs associated with the asylum system. By 1845, the medical profession had secured the superintendencies of the asylums for its members, as well as a place on the Commission appointed to inspect the workings of the madhouses. But the consolidation and extension of these prerogatives continued to be obstructed in the mid-nineteenth century.<sup>96</sup>

According to Jones, there were, after 1845, three lines along which the institutions of insanity could develop: the 'social' or 'humanitarian', the 'legal' and the 'medical'. Each of these approaches to insanity

had the support of particular social interests; in consequence, the politics of insanity in the later nineteenth century became 'an affair of pressure groups'. In particular, the legal and medical professions competed to impose their own notions of insanity upon the deliberative and custodial institutions of society. The physicians were also assailed by a more diverse congerie of interests which questioned their competence to continue and increase their control over the insane. The committees of Justices who oversaw asylums, for instance, did not hesitate to prefer their own judgement to that of the medical officers on such questions as the release or detention of individuals, which the alienist regarded as of a clinical nature and his alone to decide. In some cases, the committees also attempted to intervene in the daily running of the asylum and to oblige the Superintendent to conform to their notions of adequate provision for and treatment of the insane.<sup>97</sup>

The participants in the conflict that resulted from these competing designs on the insane used more or less elaborate conceptions of madness to support their own proposals. The enemies of medical control made special use of notions of the 'moral' causes and treatment of mental disorder. At the turn of the eighteenth century Phillipe Pinel had repudiated the current physicalist aetiology of insanity and the concomitant medical treatment, in favour of a view of insanity as a disturbance of 'the spirit' whose remedy lay in kindness and appeals to the sufferer's moral nature. Pinel's ideas were introduced into Britain by Samuel Tuke at the famous York Retreat, and enjoyed a great vogue in the early nineteenth century.<sup>98</sup> In the 1850s and 1860s too 'moral treatment' remained as a resource for those who wished to cast doubt on the relevance of professional medical skills to the treatment of the insane. Arlidge complained in 1859 that stress upon moral means of treatment had weakened the position of the alienist because such methods 'can, to a greater or lesser extent, be carried out equally by an unprofessional man. It is therefore not surprising that the importance of the medical attendant is little appreciated, and that the value of medical treatment is little heeded'. A result of the rhetoric of moral treatment, Arlidge continued, 'had been to strengthen the conviction of 'magistrates, like other mortals', that 'medical superintendents, considered in their professional capacity, are rather ornamental than essential members of an asylum staff'.<sup>99</sup>

In the early decades of the nineteenth century British alienists responded to these threats to their professional privileges by trying 'to demonstrate that insanity was in fact caused by biophysical variables', because a 'somatic interpretation of insanity would place it beyond dispute within medicine's

recognized sphere of competence'.<sup>100</sup> An early example of this strategy is provided by William Lawrence, then surgeon to Bethlem Hospital, who related a spiritualist theory of madness to non-medical treatment and declared that the rights of the physician were best protected on the view that 'the various forms of insanity ..... are only evidences of cerebral affections ..... in short, symptoms of diseased brain'.<sup>101</sup> During the 1830s and 1840s phrenology supplied a detailed theory of the dependence of mind upon the brain which similarly conduced to a somatic view of insanity and therefore to the interests of alienists, many of whom actively supported the creed.<sup>102</sup>

After 1845, while the need to pursue the same polemical ends remained, the necessity of somewhat renovated means was seen. In the 1850s phrenology was increasingly discredited as a serious physiological doctrine and became an object of popular ridicule.<sup>103</sup> This reduced its value as an underpinning for somatic mental pathology. Browne, himself a one-time phrenological writer, informed his mentor George Combe in 1857 that if he were to state his views of the dependence of mental disorders on cerebral dysfunction in an explicitly phrenological language, he would provoke incredulity. He had therefore attempted to preserve the 'spirit' of phrenology, or, rather, the basic contention of the dependence of mind on organisation, while drawing upon more varied resources to sustain his contentions. In this context, he argued that the new physiology and anatomy of reflexion was indispensable:

in giving a comprehensive sketch of the morbid phenomena of the Nervous System, in other words, of the whole range of Insanity, it is absolutely necessary to adopt a Composite System including Bell's, Marshall Hall's ..... etc. discoveries and opinions.<sup>104</sup>

The 'new' mental pathology of the 1850s and 1860s was therefore mobilised to fill a gap left by the decline in the credibility of phrenology: it was a partly novel way of continuing an old argument. Laycock, whom the alienists regarded as an ally, and whom they made an honorary member of their Association in 1862, voiced a common opinion when he declared that the theoretical exercise upon which he and others were engaged, namely,

to determine how far a mental science in the true meaning of the term science is possible, and capable of practical application to mental pathology, therapeutics and hygiene, and the needs of society in general

was 'one of the highest importance to their [the alienists'] professional success'.<sup>105</sup>

In a similar vein, J.C. Bucknill had in 1857 censured John Conolly for maintaining that the 'direct appliance of therapeutical means' was of dubious value in complaints of the 'spirit'. This was to concede the case of the enemies of the alienist. The professionally proper stress, Bucknill contended, was upon the potential of recent physiology to throw light on the workings of the nervous system, and so upon the physical correlates of mental disorder.<sup>106</sup>

The concern found in Bucknill and Tuke's textbook of mental psychology to attribute all mental disorders to definite 'organic', or structural causes can be related to this interest in vindicating the rights of the physician against the claims of the advocates of moral and spiritual theories. 'At bottom', Bucknill and Tuke conceded, the physiological and anatomical points that they were discussing resolved themselves 'into the origin of thought and consciousness',<sup>107</sup> and the particular origin that was attributed to mind implied in the mid-Victorian context a recommendation about how the disorders of thought and consciousness should be treated.

This polemical goal permeated the detail of Bucknill and Tuke's pathology. Their emphasis upon the **microscopic** structure of the brain was an answer to those who pointed to the absence of gross lesions in the brains of the insane as support for the 'prejudice' that led them to regard 'insanity as the conditions of a certain metaphysical entity'. Similarly, the question of the dependence of sound mental function upon a healthy supply of blood to the brain was important because

it has been quite the custom, among a certain class of writers, to argue that the pathological changes discoverable in the brains of insane persons are secondary changes of little importance to the elucidation of mental disease. Pinel, and after him, Esquirol, unfortunately expressed this view.<sup>108</sup>

In 1865, Tuke named one particular field in which somatic pathologies of mind could be usefully deployed — in 'the persistent efforts of our profession to educate lawyers and legislators in the facts of insanity and the principles of modern cerebral psychology'.<sup>109</sup> The lawyers, in fact, were the most formidable of the rival groups with whom the alienists contended for control of the insane in the mid-nineteenth century. The legal profession was less concerned with the moral **treatment** of the insane than with a 'moral' **definition** of insanity, which fitted their preconceptions better than the rival medical

view. At the basis of this controversy lay a professional dispute over authority and jurisdiction.

The lawyers adhered to the basic notion of the common law in treating questions of insanity, that the decision to be made was on matters of fact which were within the competence of jurymen — after they had been suitably instructed by a judge. These prejudices had been formalised in the 1840s in the M'Naughton rules, which held insanity to be an affliction of the reasoning faculty that vitiated a person's sense of the 'moral character' of his action. The alienists opposed both the procedure and the reasoning of the lawyers. They argued that insanity was not a matter of the 'mens rea', or mental state of the accused, but of a physical syndrome of the nervous system which occasioned certain kinds of behaviour. The determination of whether a person's actions fell within such a syndrome lay not with the layman, but with the expert medical witness.<sup>110</sup>

The lawyers treated such claims as an attempt to encroach upon their control of the legal process and used the vast legislative influence available to them to thwart the alienists, and to protect their own view of madness. In the course of a debate in 1862 on a Bill to alter the procedures whereby insanity was established, Westbury, the Lord Chancellor, declared that

the introduction of medical opinions and medical theories into the subject has proceeded upon the vicious principle of considering insanity as a disease; whereas the law regards it as a fact that can be ascertained by the evidence in like manner as any other fact.

The latter view preserved the interests of the lawyer, whose professional skills alone would be needed to supplement the common sense of the juror. If, however,

you begin by telling them [the jurors] this is a most peculiar question, lying within the province of medical men, then undoubtedly they will consider that it is a matter removed out of the ordinary sphere of their knowledge, and that they must accept the opinions of others in order to come to a determination upon it.<sup>111</sup>

The alienists replied by reiterating their somatic view of insanity with renewed vigour.<sup>112</sup> Laycock in 1862 detailed the clash between legal and medical conceptions of madness, and explained why the issue was of such 'deep interest to the medical profession'. The legal and medical theories, when put into practice, produced different 'careers' for miscreants: 'Medicine says, restrain and cure the insane and imbecile offender against the law; law says, hang, imprison, whip, hunger him'. The medical



definition would therefore channel a larger number of individuals into the therapeutic institutions — ie. the asylums — while the legal directed them into the prisons. The former course would have strengthened the appeals of the Medical Superintendents for enhanced resources, and this possibility provided the material basis of the alienists' campaign against the lawyers. In the course of the mid-nineteenth century the alienists had already succeeded in channeling a large number of what had been previously regarded as 'paupers' into the asylums. Thereby they had increased the proportion of Poor Law expenditure on their institutions from 8% in 1861 - 65 to 12.4% in 1876 - 80. Now they looked for similar gains among the prison population.<sup>113</sup>

The conflict between alienists and lawyers formed part of a more extensive pattern. The upper echelons of the legal profession, together with the Established clergy, were one of twin pillars of the old order in Britain. The alienists, in contrast, were one rather despised section of a profession which had not yet lost the stigma of being a mere 'trade'.<sup>114</sup> Other sections of the medical profession were engaged in similar struggles for recognition in the mid-nineteenth century. Often, the spokesmen of the alienists claimed to represent medicine as a whole.<sup>115</sup>

Laycock had in 1846 announced the appearance of the medical profession as a new force in the state.<sup>116</sup> However, because of its novelty, he complained, medicine was underprivileged in comparison with the older professions. Laycock singled out for complaint the fact that the medical profession, in contrast to the Law and Church, had only one parliamentary representative to defend and to promote its interests.<sup>117</sup>

This social distinction was matched by a cultural divide. While the lawyers and the clergy justified their power and privilege in terms of a scholastic education at the ancient universities, the medical profession had been intimately connected with science throughout the nineteenth century. In Laycock's words, medicine had acted as 'parent and protector' to a multitude of sciences:

Botany, chemistry with its subdivisions, galvanism and electricity; and natural history, in all its branches, have had physicians as their earliest and most constant cultivators. All these, without exception, have arisen directly from the profession.<sup>118</sup>

In the years to come medicine was to capitalise upon this historical connection by using 'science' to press its claims against established elites.

More than twenty years after Laycock wrote, E.D. Mapother, Professor of Anatomy and Physiology at the Royal College of Surgeons in Dublin, iterated an almost identical complaint. In a prize-winning essay, he insisted that the medical profession in Britain 'does not hold the rank to which its importance entitles it'. That importance derived largely from the intimate relations between medicine and science: the medical profession was 'the largest community of educated persons in the United Kingdom.'<sup>119</sup>

Again, the issue of Parliamentary representation was crucial. In the course of the 1840s and 1850s physicians had agitated for the right to elect MPs who would serve their interest. This usually took the form of demands that the medically-biased London University and the various licensing bodies for physicians should have the right to send Members to Westminster; it was argued that this would be no more than an extension of the privileges already enjoyed by Oxford and Cambridge. However, the 1867 Reform Act had disappointed these hopes: London University was granted representation but, Mapother complained, this was an inadequate guarantee of the interests of medicine. While 'the clerical and legal professions and commercial and other callings have very many [Parliamentary] advocates', medicine still lacked an effective voice. It had yet to be accepted as a true estate of the nation.<sup>120</sup>

At issue were material resources as well as status. The government could provide a wide range of employment for medical men. In this respect the asylum system was the model for the wider ambitions of the medical profession: in other fields comparable institutions could be created which would extend the power and perquisites of medical men.<sup>121</sup> Moreover, just as the view that mental disorders were ultimately physiological defects had legitimated the pretensions of physicians in the field of insanity, so the theory that other 'social problems' had somatic causes was implicated in the larger strategy of the medical profession. A diseased society needed the application of the proper professional skills if it were to recover and flourish.

The continuity between the two contexts was strengthened by the fact that the advocates of physicalist theories of madness tended also to propound a physiological approach to other issues. *The Journal of Mental Science* was an important organ for the dissemination of the latter as well as the

former view.

Thus J.B. Thomson, Resident Surgeon at Perth Prison, set out in 1870 to establish the existence of a 'criminal class' in society. This class infested the cities and was characterised by an inferior body-type and a tendency to disease. Moreover, their 'moral nature seems equally diseased with their physical frames'. Thomson anticipated some opposition to his claim that character traits were hereditary but argued that the somatic pathology that had been developed to account for madness could be extended to cover other deviant behaviour.<sup>122</sup>

He maintained, as did other writers in this genre, that epilepsy, alcoholism, insanity and criminality formed a continuum: they were all 'disorders of the mind' with similar neural causes. Since the nervous system was part of each individual's genetic endowment, it followed that its defects were also congenital. Thomson argued that society was the aggregate of the hereditary characteristics of its members; social divisions could be explained in terms of the unequal division of advantageous qualities in the population. Such schemes therefore accounted for social inferiority as well as for deviance.<sup>123</sup>

This was, in part, a naturalistic justification of social hierarchy. Thomson tempered his hereditarianism with a Lamarckian recognition of the effects of environment on congenital characteristics, but only to assert that the conditions under which one generation lived determined the capacities of its descendants:

The analogy of what happens by habits of training among some of the lower animals, proves that class habits must necessarily be transmitted to the different classes of society.<sup>124</sup>

The inheritance of inequality was therefore a biological as well as a social fact.

However, theories of this kind also had a more specific import: a pathological interpretation of deviance implied a policy of social hygiene. Thereby it identified the holders of certain skills as especially qualified for the tasks of social control. Henry Maudsley who, from 1862, edited the **Journal of Mental Science** was particularly active in drawing out these implications. In 1873 he argued that crime and other anti-social behaviour had, in common with insanity, somatic roots which were transmitted from one generation to the next. Maudsley insisted that

lunatics and criminals are as much manufactured articles as are steam-engines and calico-printing machines, only the processes of organic manufactory are so complex that we are

not able to follow them. They are neither accidents nor anomalies in the universe, but come by law and testify to causality; and it is the business of science to find out what the causes are and by what laws they work.<sup>125</sup>

If the propensity to crime was of this character then punishment, as normally conceived, would be ineffective.<sup>126</sup> Similarly, environmental measures would have little effect: education, Maudsley held, can plainly act only, firstly within the conditions imposed by the species, and, secondly, within the conditions imposed by the individual organization: can only, in the former case, determine what is predetermined in the organization of the nervous system and of the bodily machinery in connection with it .....; can only again, in the latter case, make actual the potentialities of the individual nature.<sup>127</sup>

It was only when these facts of nature were recognised that true social amelioration would be possible and man would become 'the conscious framer of his own destiny'.<sup>128</sup>

But if the criminal and idiot classes were to be reduced and the number of 'honest and well-bred operatives' increased, the task must be entrusted to those who understood the natural laws which controlled human endowment. These were 'not merely subjects for the moral philosopher and preacher' but fell within the ambit of the scientist. Maudsley proposed a programme of enquiry into the physiological bases of society as a first step to the eradication of dangerous and unproductive sections of the community.<sup>129</sup>

His *Pathology of Mind* (1879) was intended as a contribution to this project. Maudsley held that a wide range of behaviours which were normally attributed to 'moral' causes were, in fact, the result of 'neuroses': that is, of defects of the nervous mechanism. He predicted that soon society would recognise that those who suffered from such disorders and transmitted them to their children must be culled. Only so would their behavioural patterns be eliminated. In effect, he advocated a policy of eugenics.<sup>130</sup>

In this way somatic theories of mind were integrated into a wider naturalistic understanding of man and society. Most of the specific physiological and anatomical doctrines that had sustained the physicalism of the alienists were abandoned. What remained was the insistence that mind and body be inseparably coupled together with an expanded 'pathology' which included a great range of behaviour and tendencies among the consequences of a diseased nervous organisation. At this point other resources,

such as contemporary theories of heredity, supplied the remaining justification for the eugenic programme.

As the dogma became more generalised so did the interests it served. What, in one context, had articulated the concerns of a section of the medical profession came, in another, to represent the interests of a class-fraction. The eugenic movement in late nineteenth century Britain, among other things, was the means whereby sections of the professional middle-class, notably the physicians, pressed their demands for enhanced status and power.<sup>131</sup>

In this instance, therefore, somatic theories of mind became part of a strategy which employed a variety of materials to the same end: namely, to assert the amenability of social questions to 'scientific' treatment. This was one of the ways in which naturalism conduced to the goals of those who professed to wield such skills. However, this did not exhaust the uses of the model of the nervous system as the determinant of mind to the new professionals. In other fields it was invoked to legitimate particular orientations in mental science. Moreover, it became a key agent in the erosion of received notions of the relation of the spiritual to the physical. Thereby, as so often before, physicalism became charged with a political meaning.

### iii. Mind and Organism

In 1893 the Scottish philosopher Andrew Seth Pringle-Pattison wrote of recent changes in mental science. He held that until the 'last half century, or thereabouts, psychology had been an appanage of the philosophers'; as such, psychological questions had been intermixed with metaphysical issues. In contrast,

The distinguishing note of most recent psychology has ..... been insistence upon the separation of psychology from philosophy, and on the maintenance of a purely psychological standpoint. In psychology, it is argued, we have a realm of phenomena, a moving world of causes and effects, which it is our business to investigate in the ordinary scientific way, with all the resources of observation and experiment, and without any *arrière pensée* as to the bearing of our results on the ultimate problems of philosophy.<sup>132</sup>

This account bore some relation to events in Germany and in the United States in the later nineteenth century. There psychology had achieved a considerable measure of autonomy from philosophy, and had strengthened its ties with physiology and other branches of biology. However, Britain had followed a different course. While similar tendencies were *intimated*, they were not on the whole fulfilled. In consequence, both intellectually and institutionally, British psychology was regarded as anachronistic by 1900.

The 1870s saw the beginnings of a movement to attain for psychology in Britain the same kind of resources and status that it enjoyed elsewhere. The journal **Mind** was founded in 1876 as part of this endeavour. Its editor, G. Croom Robertson, complained in its first issue of the 'unprofessional character' of British psychology; of the fact that the discipline lacked the 'multitude of trained and continuous learners' found on the Continent. **Mind** was intended to still doubts about the scientific character of psychology by the exposition of modern theories of mentality. Prominent among these were those that issued from 'Physiological investigation of the Nervous System in man and animals, by which mental science is brought into relation with biology and physical science generally.'<sup>133</sup>

A naturalistic approach to mind was thus seen as instrumental to professionalisation. G.H. Lewes elaborated on this theme in 1879. Psychology would never be regarded as a science so long as its

principles were the object of dispute between the various 'schools' of mental science current in Britain. The programme which Lewes prescribed for the 'constitution' of scientific psychology involved the assimilation of psychic to biological phenomena. He defined psychology as the study of the human mind conceived, not as an individual's thoughts, but as 'the product of the Human Organism not only in relation to the Cosmos, but also in relation to Society.'<sup>134</sup>

Others attempted the same project of incorporating psychology into life science by approximating, as far as possible, the mental to the physical and the human to the animal. Henry Maudsley, in addition to his clinical concerns, was active during the second half of the nineteenth century in attempting to institute a programme of psychological research at London University. He was also a major theorist of a somatic psychology.<sup>135</sup> He declared it to be imperative that

the absolute and unholy barrier set up between the psychical and physical nature be broken down, and that a just conception of mind be formed, founded on faithful recognition of all those phenomena of nature which lead by imperceptible gradations up to [the human mind]..<sup>136</sup>

The application of evolutionary notions to psychology, made first by Herbert Spencer, played an important part in these efforts to ground mind in nature. Spencer declared in the 1870 edition of his **Principles of Psychology** that 'If the doctrine of Evolution is true, the inevitable implication is that Mind can be understood only by observing how Mind is evolved'.<sup>137</sup> He had long argued that the evolutionary perspective demanded a stress upon the continuity between mental and those other phenomena which most approximated to them in the developmental scale — namely, the 'phenomena of vital activity'.<sup>138</sup>

In particular, Spencer argued that the **reflex**, conceived as the simplest instance of an adaptation by the organism to its environment, was the type of the most basic psychical event. The fact that reflexion was intimated in the simplest organisms, which lacked any distinct nervous structure, and that this property seemed therefore to be intrinsic to living substance, showed that 'mind', in its rudimentary condition, was inseparable from the physical life which generated it. The effect of evolution was to assign separate mechanisms to the reflex function, and a corresponding specialisation of mental faculties was achieved; however, even in its highest development, mind never lost the

physiological character which its origins necessitated.<sup>139</sup>

On this view of mind, consciousness was of secondary relevance to the psychologist: the most basic reflexes were unconscious and yet 'mental'. This stood in stark and deliberate contrast with the insistence of the introspectionists on the identity of 'mind' with the activity of the self-conscious ego. Maudsley pressed home this point in his own definition of mind. He argued that the concept of mind contained two sets of particulars: the organic changes that occurred during **every** mental event and the series of states of consciousness which accompanied **some**. In both cases it was the individual event which had real existence; the universal category 'mind' was merely an abstraction from these particulars, and had only a nominal status. The notion of the ego as the subject of mental actions Maudsley attributed to 'that powerful tendency in the human mind to make the reality conformable to the idea ..... this general conception has been converted into an objective entity, and allowed to tyrannize over our understanding. A metaphysical abstraction has been made into a spiritual entity, and a complete barrier thereby interposed in the way of investigation.'<sup>140</sup>

The proper object of psychological enquiry was not an hypostatized abstraction, but either the physical or the psychical series which together constituted mental life. Maudsley did not deny the existence of 'inner psychic states'; he did, however, maintain the validity and feasibility of a mental science which ignored them and concentrated exclusively on the somatic side of 'mind', on 'the changes of matter which are the condition of its manifestation'.<sup>141</sup>

Just as Laycock gave a statement of psycho-physical parallelism that was unusual in its clarity, so Maudsley intimated a view of a truly somatic psychology that was too extreme for most of his contemporaries — even for those who shared the same general interest in a biological psychology. More usual was Lewes' view: he held, that it was a 'great mistake' to transform 'the antithesis of conscious and unconscious into the equivalent of mental and physical', and that psychology could not continue to be pursued **sui generis** but needed to be treated as 'a branch of Biology' and to be conducted according to the method 'which is pursued in the physical sciences'.<sup>142</sup> However, Lewes also maintained that a **purely** experimental, observational psychology was inadequate: resort to introspection was indispensable. What was needed was a naturalistic account of the genesis of the contents of consciousness in terms of man's past evolution.<sup>143</sup>



Such an evolutionary explanation of the individual psyche and, by extension, of the 'group mind' or culture, had already been preached by Spencer and practised by Darwin.<sup>144</sup> Their lead was followed by others, who laid the basis of a comparative psychology, as well as of an evolutionary sociology in late nineteenth century Britain. In effect, these workers retained the old faculty psychology, and merely tried to break its connection with nativism and the 'ontological school'.<sup>145</sup>

In contrast Maudsley offered a more radical departure from the conceptions of traditional psychology and pointed towards different forms of scientific practice. He showed how, on the assumptions of psycho-physical parallelism, psychology could be detached entirely from subjective notions and its attention focussed upon the objective aspects of an animal's interaction with its surroundings: upon the 'material movements' which resulted from reaction to stimuli. These 'movements' could be divided into the actions of the neural mechanism and the muscular movements, or behaviours, that resulted from these. Corresponding to these two classes of motion were two kinds of science: a neurology which catalogued and measured the activity of the nervous system, and a psychology that reported and correlated the performances which identifiable external and internal stimuli produced in the organism. These projects were not entirely distinct: indeed, each assumes the other; however it is useful to distinguish them because, while the former approach was developed into a viable field of research practice in Victorian England, the latter was merely hinted at, and only fully exploited elsewhere.

John Hughlings Jackson, in the course of the 1860s and 1870s, drew upon the same resources as Maudsley — primarily evolutionary biology and the physiology of reflexion — to produce a 'grammar' for future neurological investigation.<sup>146</sup> This language of neurology is especially interesting because it was generated not as a purely speculative exercise, but as a succession of improvisations put together to elucidate and justify current lines of investigation. Jackson's writing consequently has an *ad hoc* quality which stresses the contingency of his theoretical statements upon a pre-existent **practical** interest.

This interest lay in Jackson's efforts to provide a materialist explanation of paralytic and convulsive diseases in terms of disorders of the motor centre lodged in the cerebral hemispheres. Jackson viewed the cerebral convolutions as 'nervous arrangements representing movements',<sup>147</sup>

whose morbid discharges were responsible for convulsions, hemiplegia, aphasia, and, by extension, 'all kinds of mental disorder'. However, the hemispheres were also viewed in a different way, as the physical correlates of consciousness. Central to Jackson's achievement was his separation of his 'sensory-motor' from this 'psychical' view of cerebral function, and his insistence that the former **alone** supplied an adequate framework for neurological explanation.

His strategy was not to dispute that the cerebrum was 'for mental operations', but to deny that this aspect of the action of the hemispheres was relevant to the kind of ~~en~~quiry he was pursuing. Jackson grounded this methodological axiom upon the metaphysical principle of his former associate Thomas Laycock: given the assumption of a parallelism between mental and physical states, it was possible to admit that 'along with excitations or discharges of nervous arrangements of the cerebrum, mental states occur and yet to assert, that 'how this is I do not inquire; indeed, so far as clinical medicine is concerned, I do not care.'<sup>148</sup>

The immediate applications of Jackson's dicta did, indeed, lie in clinical medicine; but they were also to have a more general significance. Jackson's work marked the institution of the 'formal hypothesis about the relations of mental with physiological phenomena', first clearly enunciated by Laycock, as the axiom of an entirely mechanistic neuro-science. Unencumbered by subjective categories, the study of brain function could become an integral part of general physiology.<sup>149</sup>

The second line of research which could follow from a truly somatic theory of mind — the study of behaviours — did not find a Jackson in Victorian Britain to associate it with a body of concrete scientific practice. However, Darwin's *The expression of emotions in man and animals* did contain hints about the potential for a mechanistic science of behaviour that was implicit in contemporary notions of the nervous system. Darwin was concerned with the muscular contortions (chiefly of the facial muscles) which were usually 'explained' in terms of the emotional states of the subject. But Darwin suggested that they might be better understood in terms of the nervous processes common to all animals. He argued that

certain actions which we recognise as expressive of certain states of mind, are the direct result of the constitution of the nervous system, and have been from the first independent of the will, and, to a large extent, of habit.<sup>150</sup>

Stimulation of the nervous centres would, with or without consciousness, achieve certain muscular changes of a regular kind, and Darwin held that the facial and respiratory muscles were particularly sensitive to such innervation. Since these were the chief agents of 'emotional' expression, it could be inferred that 'the principle of the direct action of the sensorium on the body, due to the constitution of the nervous system, and from the first independent of the will, has been highly influential in determining many expressions.'<sup>151</sup>

Darwin did not systematically develop these hints; his remarks show that he had not fully assimilated the possibilities inherent in the concept of reflexion of explaining behaviour in entirely physical terms. He admitted that 'I have felt much difficulty about the proper application of the terms, will, consciousness, and intention', and this was reflected in an equivocal attitude toward the causal potency of psychic states.<sup>152</sup> This confusion was, in part, the result of the variety of interests Darwin pursued in this work, most of which were remote from any aim to lay the theoretical foundations of a new scientific speciality; he was more concerned with making a broader polemical point about the ubiquity of emotion, supposed by some to be a uniquely human trait, in the animal kingdom.<sup>153</sup>

Others, in different contexts, stated explicitly the principle that Darwin had implied. Most notable among these was Jacques Loeb who developed a theory of animal behaviour as determined exclusively by the physical and chemical processes active in the organism. The 'tropisms' or regular behaviours so occasioned were functional, in that they adapted the organism to the environment, although performed unconsciously; Loeb's concept was, therefore, the purest statement of the idea of unconscious intelligence that was immanent in the writings of Spencer and other theorists of the reflex action.<sup>154</sup>

However, despite these attempts to construct a naturalistic science of mind, British psychology in the late nineteenth century showed few signs of matching the Continental model. Croom Robertson confessed his disappointment in 1883: for all his efforts as editor, the pages of *Mind* reflected the continued dominance of the 'old' philosophical psychology in Britain.<sup>155</sup> Moreover, intellectual 'backwardness' was matched by material poverty and by the perpetuation of the unprofessional character of British psychology.

While by the 1890s Germany and America were 'teeming with laboratories and professional experimental psychologists, Great Britain was advancing slowly in the new science only by way of the work of a few competent men, who ..... had some independent means.' As a result of this discrepancy,

the new psychology became professionalized in Germany and America as it did not in Great Britain ..... In Germany and America there was a living to be had for psychologists in universities ..... In Great Britain it was not.<sup>156</sup>

In part this was due to hostility within the universities to the new psychology. Resistance on the part of the established introspectionist schools in the universities can be traced back to William Hamilton's attack upon the phrenologists.<sup>157</sup> In the Edinburgh context, intuitionism was the property of the entrenched academic elite; on the other hand, the naturalistic formulations of the phrenologists were used by outsider groups to attack the privilege of the old order.<sup>158</sup>

Later in the century James Martineau viewed physiological psychology in a similar way. He saw such innovations in mental science as an aspect of the pretensions of parvenus professions, especially medicine. Thus Martineau complained that, to judge from the literature of the new psychology, 'the physiologist considers himself to be treading close upon the heels of the mental philosopher, and to be heir-presumptive, if not already rival claimant, to the whole domain.'<sup>159</sup> The 'physiologist's definition of mind as a function of the organism implied that there was no basic difference between 'the facts of life as manifested through the lower grades of existence, and the facts of mind, special to our race.' This theoretical stance had practical implications: according to the new psychology 'the study of humanity constitutes only the uppermost stratum of scientific "Natural History" and should therefore be pursued by scientific means. In this way, psychology would be taken out of the province of the philosopher-theologian and placed in that of the physician-scientist.'<sup>160</sup>

However, physiological psychology raised still wider issues. There was no necessity for a conflict between the old and the new mental sciences. In Germany experimentalists co-existed with traditional metaphysicians in departments of philosophy; in America a more complete division of labour was achieved and the new psychology housed in its own institutions which did not compete with philosophy. In Britain too there was evidence in the late 1860s and 1870s that philosophy was redefining

its ambit in a way which obviated conflict with a biologically-orientated psychology.

In particular, the work of John Grote and his school showed a readiness to achieve such a renegotiation. Grote accepted the validity of physiological psychology and of comparison between animal and human mentality. However, he insisted that this left untouched a further level of enquiry which could be regarded as properly the concern of the philosopher. On this view, philosophy encompassed

the study of thought and feeling not as we see them variously associated with corporeal organization, and producing various results in the universe, but as we understand, think, feel them of ourselves and from within.

Such a study was 'something :.... of an entirely different nature, and leads to entirely different fields of speculation than the physiological philosophical.'<sup>161</sup>

Subsequent Cambridge philosophers, notably Henry Sidgwick, also insisted that philosophy inhabited an entirely different universe of discourse from the sciences, including psychology. It was possible to discuss epistemological and ethical questions without making descriptive claims about the universe. Indeed, in the culmination of this philosophical tendency, G.E. Moore's *Principia Ethica* (1903), the attempt to substitute such 'naturalism' for moral philosophy was deemed to be a logical fallacy.<sup>162</sup>

By claiming that philosophy was the study of a realm of values or of the relations between **words** rather than things, its traditional preoccupations could be preserved without yielding anything to those who called for a scientific psychology. The reasons why this option was not exploited more thoroughly in late-Victorian and Edwardian Britain, and why physiological psychology attracted such hostility, need to be sought in the wider context in which notions of mind were deployed.

The issues raised by the new psychology were 'part of one debate: the place of mind in physical nature'. In other words, the disputes over the correct principles of a science of mind formed part of the general controversy occasioned by the proclamation of scientific naturalism in Victorian Britain. On the one hand, the introspectionism of the universities with its belief that 'mind possessed certain innate powers was compatible with a faith in absolute religious, moral and social values and was usually related to one aspect of God's beneficent design of man's faculties for the world in which he

lives.<sup>163</sup>

Moreover, ever since Reid, such a nativist epistemology had been associated with a particular ontology: one which attributed the faculties of will and reason to a unitary 'principle of thought [which] we call the mind or soul of a man.'<sup>164</sup> A prime concern of this kind of psychology throughout the nineteenth century was both to insist on the independence of this spiritual entity of the body and to claim that, through its will, it was capable of influencing material events. Not only was the existence of an immortal soul thus preserved but a model was presented of God's relation to nature.<sup>165</sup>

On the other hand, a psychology which assimilated the psychic to the vital, and which questioned the causal potency of consciousness, was seen as a dangerous contribution to a thoroughgoing materialism. Thus Pringle-Pattison attacked 'German' psychology, as exemplified by Münsterberg, on the grounds that it eliminated the notion of an active, causally-effective ego, and thus contributed to a 'psychology without a soul'.<sup>166</sup>

The defence of introspectionist psychology was also a repudiation of materialism. This was evident in the writings of two of the most prominent advocates of the old psychology in late-Victorian Britain, James Ward and G.F. Stout. The latter announced his opposition to the pretensions of physiological psychology in 1891; there was, Stout claimed,

no direct means of tracing the connection between a mental fact and the corresponding physiological fact. There is a gulf fixed between the physical and the psychological of such a nature that it is impossible coincidently to observe an event of the one kind and an event of the other kind, so as to apprehend the relation between them ..... [E]ven if matter were the only real agent, psychology would nevertheless remain a field of inquiry separate and distinct from that covered by the investigation of the material organism.

In fact, Stout was not disposed to admit that material causes alone determined natural occurrences; on the contrary, he insisted that the agency of the immaterial mind provided the model for an 'animistic' explanation of all causation.<sup>167</sup> He thus secured, not only the priority of introspective data in psychology, but both the independence and the superiority of the spiritual in nature.

James Ward's psychology differed from Stout's in detail; however, both were as insistent upon the primary status of consciousness in mental science and of mind in the cosmos. Ward, however, was, at

least early in his career, not hostile to some experimental psychology: he had himself received a training in the subject in Germany. He even favoured the establishment of a psychological laboratory at Cambridge. But the social setting in which Ward operated, and to whose inclinations he generally conformed, asserted its power; the proposal was blocked by an 'opposition that identified a laboratory for the study of mind with the support of materialism.'<sup>168</sup>

The new psychology therefore suffered from the general hostility to scientific naturalism among certain sections of British society; a hostility which grew as the nineteenth century drew to a close.<sup>169</sup> However, the tendency to mingle psychological with cosmological issues was not peculiar to the 1880s; nor was this move peculiar to the enemies of naturalism. Previously a particular conception of the nervous system had been coupled with a distinctive psychological doctrine for the purpose of asserting the continuity of mental phenomena with the rest of nature. Such mind theories had, in the context in which they were employed, definite social concomitants.

#### iv. Mind and Cosmos

In the previous section the manner in which a particular conception of mind was made to yield positive and negative recommendations for mental science was described. 'Mind' was held to be inseparable from the organism and mental functions, in their simplest condition, to be reducible to the reflex. Mind was causally independent of consciousness; its workings could be referred to physiological events which were amenable to observation and experiment. Thereby fields of scientific practice were circumscribed and the possibility of an interaction between psychology and other branches of life-science established.

The converse of this process was a repudiation of more traditional approaches to mental science; in particular, metaphysical and theological questions were deemed irrelevant. Such concerns corresponded with the more general endeavour in Victorian society to distinguish science from other forms of culture and its practitioners from other groups.

This interest has been identified as a major goal of the naturalist movement in Britain. Within this strategy a physicalist theory of mind, underpinned by a simplified version of the reflex model of the nervous system, came to play an especially important role. The most sensational statement of this theory was Thomas Huxley's 'Animal Automatism'; an address which was also a declaration of the autonomy of scientific discourse.

The meeting of the British Association for the Advancement of Science at Belfast in 1874 was chosen as the forum for the announcement of the naturalist cosmology and of the professional demands associated with it. Tyndall's contribution as President to wresting 'from theology the entire domain of cosmological theory' has been described in Chapter Two. Huxley, for his part, while expressing some reservations at the boldness of Tyndall's plan, declared himself prior to the meeting to be 'at your [Tyndall's] disposition for whatever you want me to do.'<sup>170</sup> In the event, Huxley's contribution was to select one part of the 'domain of cosmological theory', mind, and to repel those doctrines which had infringed upon scientific autonomy in this field and to put forward one of his own. The result of this concerted display of infidelity was, as Tyndall later commented, that 'the parsons had something to handle' in the aftermath of the Belfast meeting.<sup>171</sup>

Laycock had in 1860 expatiated upon the obstacles to a naturalistic psychology imposed by



theological interest in theories of mind. He had concluded that 'speculative theology' had exerted 'injurious restrictions upon inquiry'. William Hamilton's response to physiological psychology exemplified this: there the 'successful application of physiological research to philosophy is denounced as subversive of truth and a reverent adoration of the Supreme Being.' In short, psychology had been put into a 'sort of theological cage'.<sup>172</sup>

Huxley's argument at the Belfast BAAS was an attempt to shatter the bars of that cage by openly defying the ground rules speculative theology had introduced into the discussion of mind. Huxley had foreshadowed this strategy three years earlier in a discussion of Descartes' *Discourse on method*. He had distinguished a double legacy for psychology in Descartes' thought: one approach found there led to an exclusive concern with consciousness; the other 'shows us .....[an] apparently very different, path, which leads..... to that correlation of all the phaenomena of the universe with matter and motion, which lies at the heart of modern physical thought, and which most people call materialism'.<sup>173</sup> This latter approach, with its stress on the mechanical character of nervous action, was, Huxley insisted, 'exactly that of the most advanced physiology of the present day', and it was this orientation that he espoused:

I hold, with the Materialist, that the human body, like all living bodies, is a machine, all the operations of which will, sooner or later, be explained on physical principles. I believe that we shall, sooner or later, arrive at a mechanical equivalent of consciousness, just as we have at a mechanical equivalent of heat.<sup>174</sup>

His concern, Huxley stressed, was not to be anti-Christian; it was rather to show that the natural sciences, including psychology, were

Extrachristian, and have world of their own, which ..... is not only "unsectarian", but is altogether "secular".<sup>175</sup>

In other words, he was trying to delimit a separate universe of discourse for the sciences, from which theological questions were, *a priori*, excluded.

Huxley continued the same exercise of carving out a secular empire for psychological science from which clerical influence was rigourously excluded in 1873. On this occasion, it was Descartes' physiology which Huxley used to make his point; this, Huxley alleged, had prefigured 'the foundations

and essence of the modern physiology of the nervous system'.<sup>176</sup> These could be contained in five principles: first, that 'the brain is the organ of sensation, thought, and emotion; that is to say, some change in the condition of the matter of this organ is the invariable antecedent of the state of consciousness to which each of these terms is applied.' Secondly, the **movements** of animals were held to be due to a change in the form of their muscle, which, in turn, was the result of a change in the shape of the efferent nerve. Thirdly, **sensations** were due to a 'motion' along the afferent nerves. Fourthly, Huxley restated the principle of reflexion: that the action of the sensory nerves could be transmitted to the efferent nerves and thereby cause muscular contraction and movement. Huxley added that 'this reflection of motion from a sensory into a motor nerve may take place without volition, or even contrary to it.' Finally, the stimulation of the brain by sensory and motor impulses induced a readiness to be moved in the same way again; this was the physical mechanism of memory.<sup>177</sup>

Taken together, these principles, Huxley argued, affirmed the necessary dependence of mental function upon nervous organisation; the sensori-motor, or reflex, model of the latter's working; and an account of how the contents of the psyche were developed by experience. They also provided irrefragable evidence for the Cartesian doctrine that animals were automata in whom consciousness did not determine action.<sup>178</sup> Where Huxley differed from Descartes was over the latter's exclusion of man from this generalisation and the subsequent resort to dualism.

Descartes had held that consciousness was peculiar to man, and that by the determination of the soul human movements were guided. Huxley contended that the principle of the continuity of nature would suggest instead that consciousness was a property common to all life and that man should be understood upon the same postulates as other animals. Man was therefore a 'conscious automaton' in whom feelings accompanied the motions of the nervous system. All conscious states were the product of molecular changes in the brain, but did not themselves cause any change in the motions of matter. The feeling called 'volition' was

not the cause of a voluntary act, but the symbol of that state of the brain which is the immediate cause of that act.<sup>179</sup>

Mostly, these principles were a familiar aspect of the neural physiology of the preceding two decades, but now they were mobilised to serve a particular polemical interest. As stated by Huxley,

they overthrew the Christian notion of the independence of the spirit, of the freedom of the will, and of the mere instrumental character of the body. His view of mind was, he remarked, bound to cause outrage in certain quarters — but then 'there are so very few interesting questions which one is, at present, allowed to think out scientifically ..... without being speedily deafened by the tattoo of "the drum ecclesiastic"' But, far from being intimidated, Huxley insisted upon the naturalistic implications the cleric might see in his argument: these might well be incompatible with Christian dogma, but, **from a scientific point of view**, they were unexceptionable. These were physiological questions to be determined by physiologists in which the clergy had no right to interfere.<sup>180</sup>

The final message of the address was clear: the clergy were warned to assuage their 'sacerdotal pretensions' and to keep away from an area science now claimed for its own. In this and in similar contexts, therefore, a physicalist theory of mind furthered a political purpose: like other aspects of the naturalist cosmology, it served to communicate the interests of one group to another as part of a campaign by the scientific profession to define its relationship with the clergy and to negotiate a more favourable social position for themselves.

In the last analysis, however, the dispute between scientists and clerics was not about the formal boundaries that should exist between them but about control of social institutions and of the resources to which they gave access. These controversies were part of the wider conflict in mid-Victorian society between new and established elites. These conflicts have been described above together with the alliance between political radicalism and scientific naturalism to which these gave rise.<sup>181</sup>

Psychological issues played a prominent part in the rhetoric of this movement for **two** reasons. Firstly, the nature of the human mind and its connection with the body were major contentions between naturalistic and orthodox writers. The answer to such questions was held to have crucial ethical consequences. Secondly, the human was regarded as mirroring the cosmic in this respect. According to one influential school of natural philosophy, just as the soul was independent of but controlled the body, so the divine spirit transcended but dominated nature.

In company with their eighteenth century predecessors, Victorian radical and naturalistic thinkers maintained a contrary view of mind and matter. In essence, they maintained that mind and matter were inseparable: there was no transcendent God nor any other spiritual entity independent of matter. The

advocates of this view could draw upon a monist tradition which had existed in Europe for at least two centuries.<sup>182</sup>

Thus Laycock invoked such an ontology to underpin his physiological psychology. He wrote in 1860 that God was the 'immanent and ever-operating cause in nature.'<sup>183</sup> Such language, J.C. Bucknill had warned, was too close to the pantheism of Spinoza and to Leibniz's monadology for comfort. Laycock's arguments threatened to make the concept of God as an external directing agent otiose because they attribute mind and intelligence to all matter undergoing organic changes in a definite manner for a predetermined purpose. If intelligence is attributed to all matter in a state of organic development ..... it is impossible ..... to deny the attribute of intelligence to inorganic matter also.<sup>184</sup>

Ultimately, on such principles, the notion of God, the personal Lawgiver — 'whose Will is made known in what we call the laws of nature' — would give way to one which located the directing 'teleological' principle in the basal structures of matter itself, whether these be called 'monads', 'cells' or 'atoms'.<sup>185</sup>

The same aim was detected by its enemies at the heart of the strategy of scientific naturalism. In the words of A.J. Balfour, the outcome of the naturalists' definition of 'the place which Mind, in its higher manifestations, occupies in the scheme of things' was the 'deposition of Reason from its ancient position as the Ground of all existence, to that of an expedient among other expedients for the maintenance of organic life'.<sup>186</sup> More precisely, the strategy of naturalism was to depose Reason as a principle that was external to the material world, and yet determined its course, whether through the soul's guidance of the body through volition or God's control of nature by his will.

The cosmological point had social implications. The corollaries of the doctrines of an imperial God who transcended nature and of the separate existence of the soul, John Morley wrote, were the subjection of the individual conscience to a superior power; the strengthening of a theocracy that battered on fears of judgment in a future life; and an ethics which equated right and wrong with conformity or disobedience to the divine will. The enlightened view, on the other hand, confined mind and conation to man, and lodged him in the nexus of physical events. From this basis a truly humanistic ethics and politics could proceed.<sup>187</sup>

Clifford's 'Body and Mind' exemplifies this transition from the physiological and psychological to

the cosmological and so to the social and ethical; it also affords evidence of how the broader political interests of radicalism and the more particular aspirations of the scientific profession for enhanced cultural authority were related. Science, Clifford argued, had more than a right to speak with a distinctive voice on the subject of mind and body: its pronouncements on the issue were definitive and foreclosed upon further controversy. The secular tendency of scientific thought was toward a unification of the three great fields of human knowledge, physics, biology and psychology; toward the intertwining of these 'great threads, into which all the little threads have been twined ..... into a single string.'<sup>188</sup>

In an elegant summary of the major physiological doctrines of reflexion, Clifford presented a view of the nervous system as a closed circuit in which sensory impulses alone were sufficient to elicit motor responses. At some stage in this mechanical process, psychic phenomena began to occur in parallel with the nervous; Clifford's contention was that this parallelism was exact and all-inclusive. Just as the sensations which impinged upon the ganglia through the afferent nerves were separate and multitudinous, so consciousness was an incessant flow of atomic feelings; just as the occurrence of two such stimuli in close succession created a physical link between them, so two concurrent mental events were connected in memory. The capacity of the nervous system to react to stimuli through lower or higher levels of organisation — by the reflex action of the spinal ganglia, or by those of the hind-brain or mesocephalon, or, in the case of some actions, through that of the hemispheres— was reflected in the division of consciousness between acts of which the mind was unaware, acts of which it was aware but did not feel that it had determined, and acts which seemed to result from acts of volition.<sup>189</sup>

Clifford argued that this account exhausted the contents of consciousness; he rested upon the authority of Locke and Hume to support his claim that there was nothing in the psyche but a 'stream of feelings such that each of them is capable of faint repetition, and that when two of them have occurred together the repetition of one calls up the other'. Each of these psychic facts had a physical parallel; but these formed only part of the total sequence of physical events which comprised the organism's interaction with the environment: while every fact of consciousness had a parallel in some disturbance of the nervous system, 'disturbances of my nerves may exist which have no parallel in consciousness.'<sup>190</sup>

Consciousness was therefore only the shadow of a portion of the uniform processes of the physical world. There was no need to suppose anything more than the action of the laws of mechanics to account for the motions of organic bodies. Neither 'will' nor any other state of consciousness was a 'force' because it could not, in itself, influence the relative positions of matter; that could only be achieved by other matter.<sup>191</sup>

Moreover, the dependence of consciousness upon material organisation allowed its place in the unity of nature to be closely defined. The human psyche paralleled a peculiar, rather complex, form of physical organisation; it followed, said Clifford, that a more limited, but essentially similar, phenomenon was the concomitant of more rudimentary nervous systems and existed even in beings which lacked a distinct nervous system. The evolutionary hypothesis precluded the view that at some stage in the development of life an absolute distinction between conscious and unconscious should appear; therefore, 'even in the very lowest organisms, even in the Amoeba which swims about in our blood, there is something or other, inconceivably simple to us, which is of the same nature with our own consciousness, although not of the same complexity'. This conclusion was basically a statement of the same panpsychism at which Spencer and Darwin had arrived by somewhat different routes; however Clifford went on to draw out the full implications of the argument, and to locate his views in a philosophical tradition which refused to recognise any final distinction between mind and extension. Because protozoa evolved originally from inanimate matter, 'we are obliged to assume, in order to save continuity in our belief, that along with every motion of matter, whether organic or inorganic, there is some fact which corresponds to the mental fact in ourselves.'<sup>192</sup> 'Mind' and 'matter' were the phenomenal expressions of a single noumenal substance: 'the reality which underlies matter, the reality that we perceive as matter, is that same stuff which, being compounded together in a particular way, produces mind.'<sup>193</sup> Clifford admitted that this was speculation, but maintained that it was speculation fully sustained by contemporary physiology.<sup>194</sup>

The implications of such doctrines extended beyond merely psychological issues: they ruled out the notion of an 'immaterial mind' as self-contradictory, and so dismissed the possibility of a transcendent spirit whose will shaped natural events. So far as human knowledge extended, Clifford argued, no intelligence or volition had been concerned with events in the cosmos, save that of the

animals which inhabited its planets.<sup>195</sup> There could, therefore, be no external direction of natural or human events.

Tyndall, four years later, provided a concise and historically illuminating summary of the principle of the self-sufficiency of nature that was made to follow from the doctrines of panpsychism and of the immanence of the mental in the material. Robert Boyle, in the seventeenth century, had, Tyndall reported,

regarded the universe as a machine; Mr. Carlyle prefers regarding it as a tree ..... A machine may be defined as an organism with life and direction outside; a tree may be defined as an organism with life and direction within. In the light of these definitions, I close with the conception of Carlyle. The order and energy of the universe I hold to be inherent, and not imposed from without – the expression of fixed law and not of arbitrary will.<sup>196</sup>

The practical consequence was that men should guide their individual and social actions by reference to these fixed laws, and not by appeals to the will of an imaginary super-mind that had assigned a destiny to humanity. Culturally this meant an elevation of natural science to a position of unique authority: the 'domain of science', properly viewed, included, according to Clifford, 'all possible human knowledge which can rightly be used to guide human conduct'.<sup>197</sup>

It was for the scientist to offer guidance, not only on technical, but on moral issues. The role of science extended to the creation of a new social ethic founded upon a naturalistic understanding of man, which held that human circumstances were determined by human efforts, but which recognised that these efforts must operate within the constraints of natural laws.<sup>198</sup>

The incorporation of mind, the directing, purposive aspect of reality, into the sensible world could, therefore, lead to a naturalistic ethics. This could, as the mid-Victorian Radicals hoped, supply a rhetoric of liberation from the shackles placed upon the individual conscience by theological dogmas of the rule of God. But, when liberty threatened to run to excess, there were the 'laws of nature' to supply the agent of restriction which divine will had once provided. While the former aspect of naturalism figured prominently in the political polemic of the 1870s, the latter received the greater emphasis in the last two decades of the nineteenth century. In either case, the right to speak authoritatively on social issues was denied to traditional, clerical, elites and assumed by new, 'scientific', professionals.

## Conclusion

This chapter has shown how a natural object, a particular conception of the nervous system, became the vehicle for a variety of social interests in nineteenth century Britain. Within this diversity there was a certain unity: each of these interests was in some way related to the differentiation of a professional middle-class during this period. In particular, the closely related scientific and medical professions were predominant in the formulation, interpretation and employment of these physiological and psychological doctrines.

In fact, these events epitomised the general pattern of these years. Naturalistic formulations of reality articulated the goals of the new professionals in highly esoteric as well as in more public contexts. The next chapter considers an example of the former where the creation of areas of scientific practice was the dominant concern. The final chapter returns to the wider social meaning of naturalism in late-Victorian Britain.



## CHAPTER FIVE: Evolution, Ontogeny, and the Natural System

### Introduction

The theory of evolution was a central aspect of scientific naturalism. In the polemic of the naturalist movement, the Darwinian theory was presented as the leading example of how science could explain phenomena 'mechanistically' and without resort to teleology. Darwin was aware of the broader cosmological implications of his ideas, but did not stress them in his own writings. Rather, he wrote for other men of science with the express aim of persuading them to accept his theory.

His main argument was that evolution by natural selection collated and explained a mass of current biological practices and assumptions better than any other theory. Its adoption would therefore bring greater order to existing knowledge; moreover, it would indicate more clearly the paths that future enquiry should take. For the most part, the potentials inherent in Darwin's work for the underpinning and orientation of scientific activity were not taken up by his contemporaries; the exploitation of most of them awaited the 'new synthesis' of the early twentieth century. However, there was an exception.

Evolutionary concepts played a major part in British and Continental morphology in the later nineteenth century. In particular, a version of the theory of descent was made the paradigm within which the relatively new science of embryology proceeded in Britain between 1870 and 1890. The reasons for this rapid assimilation of evolutionary thought by embryologists lay in the amenability of their prior conceptions to interpretation in 'Darwinian' terms; they appropriated only those parts of Darwin's work which they could use with minimal change in their existing procedures.

T.H. Huxley was the dominant figure in establishing those procedures in the twenty years prior to the publication of the *Origin of Species*. Huxley was also, from the 1860 meeting of the BAAS onwards, the main public champion of the Darwinian theory. However, he was among the last to agree to the amalgamation of evolutionary and embryological concepts. Indeed, Huxley fought a stubborn rearguard action against the tendency to do so in the 1870s.

This chapter examines these events both through Huxley's work and through that of those who claimed to have improved upon his efforts. These insisted that in the Darwinian theory they had discovered the true fulfilment of Huxley's programme for the science of form and development.

Through their applications of these insights they created the characteristic mode of embryological investigation in late-nineteenth century Britain.

The first section considers the resources for morphology available in Britain in the first half of the nineteenth century. The second shows how Huxley chose between these to produce a programme for the science. The third discusses how Darwin demonstrated the compatibility of the main elements of this programme with the theory of descent. The fourth and fifth discuss how morphologists developed these suggestions in the 1870s and 1880s. The sixth considers the criticisms of this kind of embryology which were made in the last decade of the nineteenth century and which became increasingly important in the next.

i. Homology, the Archetype, and the Evidence of Embryology.

I am in the middle of the "Limbs" with uncommon interest – the manner in which you work out the toes strikes me as quite beautiful. Whoever would have thought that a great Cart-horse walked on four fingers!

(Charles Darwin to Richard Owen)<sup>1</sup>

British anatomy in the 1830s and 1840s displayed a split which corresponded to the near-contemporary controversy between Cuvier and Geoffroy St Hilaire in France. One faction in Britain favoured the Cuvierian method which explained structure in terms of adaptation to function. Such 'teleological' explanation, of which the leading British practitioner was Charles Bell, had a dual advantage. On the one hand, it afforded an idiom in which to express the minute interrelations between the parts of a given organ, and between the different organs of the individual, which showed how these cooperated. Its exponents used this technique to elucidate these aspects of structure to good effect. On the other hand, teleology was well-suited to the interests of natural theology. The conformity of structure to function was taken as a prime instance of divine providence in nature. Bell's major work of this genre appeared as a 'Bridgewater Treatise' which aimed to show that 'as the beautiful structure of the animal, and the perfection in the arrangement of its parts demonstrates design – so design extends to the condition of the earth; and over both there is a ruling Intelligence.'<sup>2</sup>

However, an alternative model of morphological explanation existed in Britain as in France. In Paris during the 1830s Geoffroy St Hilaire combatted Cuvier's influence on the intellectual, as well as on the institutional level. In place of Cuvier's emphasis on particular adaptations and on the minute study of particular structures and organisms, Geoffroy followed Buffon in seeking 'general laws' of morphology comparable to those of the physical sciences. In particular, Geoffroy argued that individual forms must be referred to general 'types' of organisation. These types represented the most general features of a given class of animals and were the means whereby the relations between classes could be distinguished.<sup>3</sup>

There was an extensive traffic in ideas between Britain and France in this period. Of special importance was the flow of British medical students to Paris to acquire an advanced knowledge of anatomy. Some of these brought back elements of Geoffroy's system and developed them in domestic controversies. Specifically, they tried to establish the search for general laws as the true aim of morphology.<sup>4</sup> The link

between the Edinburgh medical school and Paris was particularly strong. Robert Knox, the future head of the Edinburgh anatomy school, travelled to the French capital in 1821 to attend Geoffroy's lectures on the 'higher anatomy'. In his subsequent teaching, Knox promulgated the ideas he had learned as a corrective to the excessively restrictive teleology of Bell.<sup>5</sup>

In his writings also Knox criticised the exclusive search for the uses of bodily parts as an explanatory model in anatomy. It was not with 'animal bodies as machinery', he wrote in 1843, that morphology should be concerned, but with the body as an exemplar of a general pattern. Related animals, Knox insisted, 'are formed upon one plan.'<sup>6</sup>

Among Knox's students was Richard Owen who was to become a leader of British anatomical thought in the 1840s. Owen had visited Paris on Knox's advice in the 1830s. There he studied under Cuvier, an experience which was reflected in his later monographs on individual species. However, Owen came away with the conviction that the resources of teleology as an explanatory principle needed to be supplemented. His decision was reached as a result of the technical problems, especially in taxonomy, which confronted the morphologist in the 1840s.

Cuvier had admitted only 'special homology' as a means of explaining the affinities that existed between animal structures: such similarities were to be seen solely in terms of comparable adaptations to alike uses. The difficulties of this narrow notion of homology were impressed on Owen in his work on the zoological classification of the osteological part of the Hunterian Museum. He complained at the unfortunate effects of the historical accident whereby the structure of animals had been studied piecemeal in accordance <sup>with</sup> of the special concerns of veterinary surgeons, ornithologists, ichthyologists and others. As a result, no common nomenclature existed for the correlation of the forms of various animals in a comprehensive classificatory schema. While this confusion of terms persisted, Owen maintained, anatomy could hardly call itself a science.<sup>7</sup>

A common terminology had to be underpinned by a much more elaborate conception of the affinities of animals than the 'immortal Cuvier' had allowed. Specifically, the meaning of the term 'homology' needed to be extended: the identification of such homology was to supply the grounds for the application of the same name to the parts of different animals. Owen entirely excluded the functional similarities upon which Cuvier had laid sole stress from his definition of homology; such

resemblances were merely 'affinities'. Owen confined the term 'special homology' to a correspondence of a part or organ which was 'determined by its relative position and connections, with a part or organ in a different animal; the determination of which homology indicates that such animals are constructed on a common type.' A 'higher' relation of homology was where a part stood in a similar relation to a fundamental type upon which a larger group of animals was constructed; this Owen called 'general homology'. A further category which he distinguished was 'serial homology'. This was best illustrated in the organisation of the vertebrate endoskeleton where a series of essentially similar segments succeeded one another longitudinally and where particular segments had been modified to perform some function.<sup>8</sup>

The 'explanation' of all these homologies lay, according to Owen, with the assumption of an 'archetypal' form that incorporated the most general structural features of a class. All actual members of that class were variants of this type. Owen's most notable effort to identify such an archetype occurred in the course of his work on the vertebrate skeleton. He had concluded that vertebral structure was based upon an 'ideal typical vertebra'. This vertebra consisted of parts which varied within any animal and between different groups; nonetheless, Owen held, 'certain parts of each segment do maintain such constancy in their existence, relation, position, and offices, as to enforce the conviction that they are homologous parts, both in the constituent series of the same individual skeleton, and throughout the series of vertebrate animals'.<sup>9</sup>

The form of the spinal axis and of all its appendages were such modifications of the ideal vertebra. Owen revived the doctrine of Geoffroy and Oken that the vertebrate head could also be viewed in this way. The assertion rested upon a certain dissection of the bones of the head which, Owen argued, allowed the normally fused parts of the skull to be referred to the segments of the vertebral type.<sup>10</sup>

Because of this connection of the particular to the general within individuals and between individuals of the same class, it was possible to represent diagrammatically the plan which underlay a multitude of diverse forms. All vertebrates, Owen concluded, were variants of one such archetype. In Owen's hands, this archetype was more than a device to correlate a mass of evidence: it revealed important truths about the nature of the organic world.

The vertebrate skeleton was only one instance of a wider unity in the structure of living things. Its general features — the repetition of a basic structure varied to meet special needs — were as well exemplified in the rings of the centipede and annelid and in the radii of Echinoderms. The cause of the repetitive or 'vegetative' aspect of form Owen identified as a 'polarizing force', analogous to that found in inorganic crystals; its influence was most evident in the lower animals. The vegetative force acted concurrently with a second 'adaptive' or 'special organizing force'. Owen identified this latter force with the Platonic 'demiurge' which imposed diversity and specificity upon the uniform and general structures produced by the polarising force.<sup>11</sup>

The existence and operation of these laws had still wider implications. The existence of ideal schema proved, Owen argued, that mind had preceded the material world. He wrote that the 'recognition of an ideal Exemplar for the Vertebrated animals' showed that

knowledge of a being such as Man must have existed before Man appeared. For the

Divine mind which planned the Archetype also foreknew all its modifications.<sup>12</sup>

Thus both the power of God and anthropocentrism were saved.

In effect, Owen was trying to reconcile his style of morphology with the demands of natural theology. In challenging conventional teleology, he laid himself open to the charge of unorthodoxy.<sup>13</sup> To counter this, he argued that his system provided a fuller and more compelling demonstration of God's action in the world. Owen redefined teleology: the purposiveness of the organic world lay in the **causes** active in it, not in any particular effects. While there was design in nature, it was reconcilable with the notion that God acted by law rather than by *ad hoc* expedients. If natural laws were conceived as 'God's ministers', then the claims of natural theology and of scientific explanation could be reconciled.<sup>14</sup>

After Owen's death, Huxley assessed the relative value of the different parts of his system to subsequent morphology. Huxley acknowledged the importance of the concept of the 'archetype' to subsequent developments. However, he criticised Owen for neglecting the class of evidence which gave this notion its fullest meaning. This was 'the study of the manner in which things acquire the structure which they possess'; that is, embryology.<sup>15</sup>

Huxley referred, in particular, to the kind of embryology pioneered by Karl Ernst von Baer in

the first half of the nineteenth century. Von Baer had taken from Cuvier the classification of the animal kingdom into the four classes of vertebrates, articulata, molluscs, and radiates; however, he tended to conceive these as morphological types rather than as the main forms of functional adaptations. His innovation was to relate the concept of the 'archetype' to the facts of development. Von Baer argued that 'typical' features were most evident in the early stages of embryonic growth: the dominant characters of type could 'be traced down to the lowest stages of organization'.<sup>16</sup>

In fact, Huxley did not give Owen his due in this respect; nor was he justified in claiming that he had introduced von Baer's ideas into British morphology. As early as 1837, the Edinburgh physician and histologist Martin Barry had used von Baer's new embryology to argue the basic point of the transcendental anatomists. This was that form could be regarded independently of function.<sup>17</sup>

Barry went on to argue, as had von Baer, that homology of form was a consequence of homologous development. Barry also followed von Baer in denying that animals 'recapitulated' the adult forms of lower beings in their development; however, creatures which developed from the same archetype would have homologous **embryonic** forms. Barry represented several of the 'ideal types' to which certain animals approximated in the early stages of growth diagrammatically.<sup>18</sup>

In his use of von Baer's embryology, Barry came close to Huxley's later position and so anticipated many of the main doctrines of later British morphology. In particular, Barry realised the potential taxonomic significance of embryological evidence; indeed, he asserted that the 'only sure basis for classification is -- not structure, as met with in the perfect state, when function tends to embarrass, but -- the **history of development**'.<sup>19</sup> In a later paper, Barry showed how from the pattern of embryonic affinity and divergence the successive orders of zoological classification could be constructed.<sup>20</sup>

Owen too was acquainted with von Baer's doctrines. He stated in 1843 that development was a process of gradual divergence from an initial formal unity. In consequence, the forms of eventually widely different animals might have much in common early in development.<sup>21</sup> But Owen's interests in embryology lay in a particular direction: he was concerned with the parallel that was supposed to exist between the fossil and the embryonic series. In both sequences the same differentiation from homogeneity was supposed to appear.<sup>22</sup> Owen was less impressed by the applications of embryological evidence to classification. Thus in 1846 he denied that similar development was a necessary criterion

of homology:

There exists doubtless a close general resemblance in the mode of development of homologous parts; but this is subject to modification, like the forms, proportions, functions and very substance of such parts, without their essential homological relationship being thereby obliterated. These relationships are mainly, if not wholly, determined by the relative position and connection of the parts, and may exist independently of form, proportion, substance, function and similarity of development.<sup>23</sup>

In other words, the relations between adult structures provided the main evidence of homology and, therefore, the criteria of classification. This tendency to neglect the morphological potential of embryological evidence was apparent in Owen's own taxonomic studies. As late as 1859, he provided a classification of mammals based entirely on adult forms.<sup>24</sup>

It was such neglect of the importance of embryology to classification that Huxley set out to counter in the 1840s and 1850s. In those decades he produced a programme for morphology. By example, rather than by direct prescription, Huxley tried to show the lines upon which the discipline should proceed. Central to this programme was the need to reinterpret the notions of 'homology' and of the 'archetype' in the way that von Baer had suggested. Properly regarded, both concepts were inferences from development. As a result, the facts of embryology had a unique significance in the discernment of the affinities which existed between organisms.



## ii. Huxley's Synthesis

In one respect, Huxley and Owen agreed on the bases of morphology: they both looked for a unity of plan among living beings and regarded this as the basis of taxonomy. During his voyage on the *Rattlesnake* Huxley wrote that 'the reduction of two or three apparently widely separated and incongruous types' was 'one of the great ends of Zoology and Anatomy'.<sup>25</sup> Specifically, Huxley distanced himself from the practice of previous investigators who had confined themselves to description of detail without seeking the unity of form between types. Instead he sought, as Owen had suggested, the broad structural affinities which united particular forms into larger groups.

Thus in the first of the papers that he produced on his researches in the South Pacific, Huxley discussed the form of the Medusae family and its taxonomic connections. Earlier enquiries into this group were inadequate, he argued, because their authors had been content to state 'matters of detail' and had failed to take a general view of 'the whole class, considered as organised upon a given type, and [to enquire] ..... into its relations with other families'.<sup>26</sup> However, he took his leave of Owen when he insisted that these homologies should be sought through the study of development.

Huxley argued that the homologies among the Medusae could be attributed to the fact that their organs developed from an essentially similar two membrane arrangement. In all Medusae, the first organ to develop from the lower of these membranes was a primitive stomach; moreover, such apparently diverse kinds as Monostomatic and Rhizostome Medusae could be related on the basis of the homologous development of their internal organs and of their outer membrane.<sup>27</sup>

Huxley isolated two of these homologies. In every Medusa the two membranes came to enclose a cavity which corresponded to the future digestive tract; the reproductive organs were variously modified into histological distinct parts which Huxley called 'thread cells'. This last feature was, moreover, as typical of other classes including Polyps, Physophoridae, and Diphyidae. This indicated that these classes 'are constructed upon the same anatomical type, that, in fact, the organs are homologous'.<sup>28</sup>

Apparently unlike organisms could, therefore, be referred to the same plan when their differences could be accounted for by the 'simple laws of growth'. For instance, the stomachs of Medusae, Diphyidae, and Physophoridae were, in the adult form, dissimilar. However, they developed from a common form.

In the same way, the disc of the Medusae was homologous with the natatorial organs of the other groups. All these organs originated in all classes as 'bud-like processes of the two primary membranes, elongating and attaining the forms peculiar to their perfect state as they grow older.'<sup>29</sup>

Huxley did not attempt to explain why such homologies of development should occur. Nor did he offer an explanation of why they should have so great a bearing on classification. He was content to introduce these principles as procedures whereby morphology could contribute to taxonomy. He concluded that the five families — Medusae, Physophoridae, Diphyidae, and the two families of Polypes — were not, as had been previously supposed, distinct; they were 'members of one great group, organized upon one simple and uniform plan, and even in their most complex and aberrant forms reducible to one type.'<sup>30</sup>

In 1851 Huxley tried to extend this group to include the 'Beroidae' (Ctenophores). These he regarded as intermediate between Medusae and Anthozoic Polypes just as Medusae were a link between Hydroid Polypes and Siphomores. Huxley proposed the name 'Nemmatomorphs', or 'thread-bearers', for this class. He was chagrined to learn that a similar classification had already been proposed under the name of 'Coelentrata'. Huxley could, and did, continue to claim originality in respect of the criteria and methods whereby he had reached his conclusions.<sup>31</sup>

Huxley tried to generalise these principles in a paper on the Cephalous Molluscs published in the **Philosophical Transactions of the Royal Society** for 1853. He frankly admitted the degree to which his own recommendations corresponded with Owen's project for morphology; Huxley quoted with approval Owen's dictum that the study of individual forms was of little value unless these facts were 'made subservient to establishing general conclusions and laws of correlation, by which the judgment may be safely guided in regard to future glimpses at new phenomena.' Such a unification of observations within general propositions to guide future enquiry was, Huxley maintained, 'the true aim of anatomical investigation'. He chose the Cephalous Molluscs as a case in which to illustrate these principles. No one had previously determined their homologies 'and so furnished the only scientific basis for anatomical and zoological nomenclature'. In short, no one had arrived at a conception of the 'archetypal molluscous form'.<sup>32</sup>

The main reason for this state of ignorance was insufficient study of the development of these

Molluscs. Embryonic growth stood 'in the relation of cause to the varieties of form'; the true meaning of homology was, therefore, the detection of a community in the causal antecedents of two or more organisms. On this view the 'archetype', the general form from which all actual Molluscs were derivatives, was the abstract idea of a hypothetical starting-point for their various developments. Huxley considered two classes of Mollusc, the Heteropods and the Pteropods, which were, 'in some respects, ..... opposite poles of the archetype of the Cephalous Mollusca.'<sup>33</sup> Their embryos differed in the development of the terminus of the body and in the consequent direction of flexure of the intestine. In the Heteropods, there was a 'development of an **abdomen**, and a consequent **neural** flexure of the intestine', while the Pteropoda were characterised by 'a **post-abdomen** [ie. a visceral mass projecting beyond the anus] , and the consequent **haemal** flexure of the intestine'. These two variants, Huxley argued, were deviations from a common developmental stage at which the axis of the body was straight with all the organs arranged symmetrically in relation to a longitudinally vertical plane.<sup>34</sup>

By collating such common stages it was possible to arrive at a conception of an ideal form which would mediate all the variations of form present in the class. This would be the 'archetype' of the Cephalous Molluscs. The archetype would possess a bilaterally symmetrical form; on its neural surface would be a foot with three segments; and the haemal surface might secrete a shell. Huxley also suggested archetypal vascular and alimentary systems which incorporated the general features common to all of these Molluscs.<sup>35</sup>

Apart from constructing his archetype upon embryological evidence, Huxley distanced himself from Owen's use of the term in another way. While he accepted that the notion of general homology and of the unity of plan in living forms were indispensable working concepts to the morphologist, Huxley repudiated the wider significance that Owen had attached to these terms. In effect, he refused Owen's **explanation** of the existence of archetypal forms in nature. Where Owen's concept of the archetype was realist and transcendental, Huxley's was nominalist and empirical.

Huxley worked out the philosophical framework which he was to attach to his programme for morphology in some 'Notes on Classification' probably written some time in the late 1840s or early 1850s. He held that the 'end of classification [is] to bring our knowledge into the fewest and most

general propositions.' The idea of a zoological class was merely one of these propositions. For every group there was a 'Type-form ..... from which all the real forms are supposed to be greater or less variations'. However, this 'Type' was also only a concept devised to codense the affinities between actual forms in an economical fashion.<sup>36</sup> This instrumentalist view of the archetype left no room for the grand speculative edifice that Owen had built upon the notion.

In public Huxley dismissed such transcendentalism as superfluous, if not inimical, to proper morphological practice. All that was meant by the 'archetype' in his scheme was

the conception of a form embodying the most general propositions that can be affirmed respecting the Cephalous Mollusca, standing in the same relation to them as a diagram to a geometrical theorem, and like it, at once imaginary and true.<sup>37</sup>

The archetype was, therefore, merely a nominal construct; it was absurd to invoke Platonic ideas to 'explain' it.

Huxley's criticisms of Owen's morphology need to be placed in the context of mid-nineteenth century British biology. Huxley had begun his career as a protégé of Owen but had rebelled against his mentor. In the later 1850s relations between the two had become increasingly acrimonious. But the conflict was not merely one of personalities. Owen represented the established order in science: he controlled a large portion of the existing patronage and was trying to secure more. He was also fully assimilated to the clerical and aristocratic elites in British society: he was a confidant of William Whewell and other Anglican divines, and had access even to the royal family. As Darwin put it, Owen 'truckled to the approbation of those high in church and State.'<sup>38</sup> His commitment to natural theology is explicable in terms of the social environment that Owen inhabited; as argued above, natural theology was one of the characteristic cultural idioms of the ecclesiastical and aristocratic establishments.

Huxley, in contrast, was the leader of the outsiders in British science. He, and other parvenus like William Carpenter, strove in the 1850s to alter the social and cultural status of science. At one level, their strategy was directed at gaining control over such resources as were available for biology and at distributing them to those they thought worthy. These efforts often took the form of trying

to usurp the influence of established scientific magnates like Owen.

For example, when in 1858 there was a proposal for the reorganisation of the science museums in London, Carpenter wrote to Huxley to express his anxiety lest control of these institutions go to one man: Owen. Owen was already superintendent over all departments of natural history at the British Museum; the government had, moreover, made him Professor of Comparative Anatomy at Huxley's own stronghold, the School of Mines at Jermyn Street.<sup>39</sup> Carpenter feared that if the rest of the London museums were added to this jurisdiction, Owen would become the 'Autocrat of Zoology and Palaeontology'. Under this regime, only Owen's sycophants, like G.H. Lewes, would receive favour, while those who belonged to the other faction would be denied advancement. Concerted efforts were needed to prevent this.<sup>40</sup>

This resentment at Owen's institutional power was coupled with an attack upon his kind of science. Carpenter drew Huxley's attention in 1855 to the second edition of Owen's **Lectures on the Invertebrates**; he noted in particular a slighting reference to Huxley's work.<sup>41</sup> Carpenter urged Huxley to retaliate. More generally, Carpenter claimed that he had 'roared over [Owen's] absurdities'. It was no laughing matter, however, that such doctrines were 'put forward as a representation of the state of British research in 1855. What will the Continentals think of us?'<sup>42</sup>

Huxley himself objected especially to Owen's concessions to natural theology. Owen's theory of the archetype was, he later maintained, an effort to bring the **Naturphilosophie** of the Germans 'into harmony with [the system] ..... of the English Platonists'. A 'sublimated Theism' lay at the foundation of Owen's speculations and vitiated his concepts: while

the "Archetype" takes the position of a platonic [idea], indeed almost of an Alexandrian [logos], ..... the essentially naturalistic abstractions — "secondary causes," "forces," and "polarity" — are personified and regarded as agents.<sup>43</sup>

In other words, Owen's morphology was the equivalent of those physiological doctrines which held that life was the effect of the action of a 'plastic nature' or 'vital principle', and that this entity was of supra-mundane origin. Such transcendentalism, Huxley argued, in this and other contexts, was inadmissible because it detracted from a truly naturalistic scientific language.

In addition to their other differences, therefore, Huxley and Owen represented divergent attitudes

to the cultural function of science. Owen, as has been seen was concerned to free biology from the narrow confines which teleological explanation of the old kind had imposed upon it. However, he did not deny that science should have some theological role; indeed, in the course of the 1850s he came to insist more and more that it should. His 'explanation' of the unity of living structure was reducible to the claim that a transcendental design was evident in nature. As late as 1864, Owen continued to maintain that British scientists had always embodied in their work 'the interpretation of the Creative power, as manifested in the properties and phenomena of God's universe which we ourselves have been created with capacities ..... of interpreting.' Biology should play a special part in this endeavour: 'Of all the manifestations of Creative Power, those afforded by living things affect our finite comprehension soonest and strongest with a sense of directness of the Maker's operations.'<sup>44</sup> The unity of plan evident in the animal kingdom was one such support of religion and, therefore, of the moral and social values which Christian belief was supposed to uphold.

Huxley, in contrast, insisted that a separation of scientific and theological discourse was a condition of the secure establishment of science. This became one of the major themes in the polemic of the naturalist movement. His concept of the archetype as a device framed to meet certain technical needs in morphology was meant to rid the notion of all transcendental implications. By insisting on this empiricist and nominalist definition, Huxley hoped to secure the integrity of one aspect of scientific culture.

Huxley's stress on the importance of embryology to morphology can be related to another aspect of professionalisation: namely, the internal differentiation of an initially inchoate biology. In Germany the type concept, deployed in close conjunction with embryological materials, had been used by scientists like Karl Gegenbaur (1826 - 1902) to distinguish morphology from physiology and to establish its principles and practice.<sup>45</sup> Huxley was an overt imitator of this German model.

He deplored the neglect of German embryology in Britain and tried to remedy it in 1853 by publishing a translation of some of von Baer's writings. These he described as 'the deepest and soundest philosophy of zoology, and indeed of biology generally'. Apart from Huxley himself, only Carpenter among British scientists had appreciated the morphological significance of these discoveries.<sup>46</sup> Huxley's own work was designed to demonstrate the potential uses of developmental evidence, as conceived by

von Baer and his interpreters, in a programme of morphological research comparable to that which had been established in Germany.

The three main aspects of this strategy were given their fullest exposition in Huxley's 1859 Croonian lecture 'On the Theory of the Vertebrate Skull'. There a critique of 'transcendental' notions in morphology and a defence of the sufficiency of empiricism were combined with an exposition of the morphological applications of embryology. Huxley's expressed aim was two-fold: first to establish the true homologies of the skull and, secondly, to consider the validity of the theory originated by Goethe and Oken, which Owen had revived, that the skull was a modified vertebra.

The lecture was usually remembered as a refutation of this doctrine. However, it was also frequently cited, even in the early twentieth century, as the foundation of the modern morphology of the skull and as an important landmark in morphology generally. This response was immediate and widespread and it makes 'On the theory of the Vertebrate Skull' a document of particular interest in discerning the state of British morphology at the eve of the Darwinian era.

Huxley began by insisting in the strongest terms on the proposition which had formed the core of his past work, that animals shared a community of organisation. Indeed, he made this principle the feature which distinguished modern biology from that of earlier times because it showed that living things conformed to law-like relations and proved that 'there is really nothing aberrant in nature; that the most widely different organisms are connected by a hidden bond; that an apparently new and isolated structure will prove, when its characters are thoroughly sifted, to be only a modification of something which existed before.'<sup>47</sup>

Nowhere was this unity of plan more evident than in the structure of the vertebral column, the different parts of which displayed such an obvious homology that the student acquired the notion of 'a vertebra in the abstract .....; the conception of an ideal something which shall be a sort of mean between these various actual forms, each of which may then easily be conceived as a modification of the abstract or typical vertebra'. Such a view seemed especially applicable to the skull which could be viewed as a version of the 'ideal' vertebra that had undergone more modification than the sacrum or the coccyx in response to the special needs of the anterior, cephalic end of the body. Because it appeared, at first glance, so plausible, this theory had remained current, but unverified, for over half a century.<sup>48</sup>

Huxley insisted on separating the general question of whether the skulls of the vertebrates were built on some common plan from the particular one of whether this plan was based upon a modified vertebra. He maintained that these two questions were of unequal interest to the morphologist. The first was fundamental. Huxley explained that unless 'it can be shown that a general identity of construction pervades the multiform varieties of vertebrate skulls, a concise, uniform, and consistent nomenclature becomes an impossibility, and the anatomist loses at one blow the most influential of stimulants to research.' On the other hand, the second question, 'though highly interesting, might be settled either one way or the other without exerting any very important influence on the practice of comparative anatomy.'<sup>49</sup>

From the outset, therefore, Huxley stressed the practical implications of the issues under consideration. He regarded the demonstration of a community of structure between vertebrate crania as important because it served the technical interests of the morphologist: it provided a basic framework of shared names as a point of reference from which the anatomist could proceed to identify homologous structures in different animals, and so establish the relationships between them. Owen had pursued a similar interest through the vertebral theory of the skeleton but he and others had also tainted the hypothesis by associating it with transcendentalist notions. In any case, it was easy enough to find alternative ways to order the anatomical data. Consequently, while Huxley exerted himself to provide an affirmative answer to his first question, the second he answered emphatically in the negative. But by far the greater effort was expended in attempting to demonstrate unity of plan by the detection of homologous structures in a wide variety of types.

Huxley was also concerned to exemplify and to commend certain methods for the study of these homologies that were familiar from his earlier work. The morphologist could either study a series of the skulls and vertebral columns of adult animals, 'determining, in this way, the corresponding parts of those which are most widely dissimilar, by the interpolation of transitional gradations of structure'; or he could trace back the skull or vertebral column to their earliest embryonic states and ascertain 'the identity of parts by their developmental relations'. These methods were complementary; but given the relative neglect of embryology by British morphologists, Huxley emphasised the indispensable nature of the latter.



For seeing that living organisms not only **are**, but **become**, and that all their parts pass through a series of states before they reach their adult condition, it necessarily follows that it is impossible to say, that two parts are homologous or have the same morphological relations to the rest of the organism, unless we know, not only that there is no essential difference in these relations in the adult condition, but that there is no essential difference in the course by which they arrive at that condition.

The study of the gradations of structure of adults might

have the utmost value in suggesting homologies, but the study of development alone can finally demonstrate them.<sup>50</sup>

Having established aims and methods, Huxley addressed the question of the unity of cranial organisation by a study of the skulls of sheep, ostrich, turtle, and carp, each of which stood as the representative of one of the major vertebrate classes. His procedure in the early part of the paper was to compare adult skulls in order to identify their homologies. At crucial points, he resorted to embryological evidence to settle doubts.

For example, in discussing the affinities of the avian and mammalian skulls, Huxley noted that the **os quadratum** in birds — a bone which applied externally to the skull and articulated with the petrosal and squamosal<sup>51</sup> — was generally regarded as the homologue of the mammalian tympanic bone, because both were connected with the tympanic membrane. He regarded the evidence for this conclusion as highly dubious, but recognised that the 'method of gradations', the study of adult structures alone, was incompetent to settle the issue; resort to the 'method of development' was needed.<sup>52</sup>

Huxley complained that Reichert had applied this criterion to the determination of the homologies of the avian quadrate twenty years previously, and Rathke had confirmed his results, but their findings were ignored. In both birds and mammals a cartilaginous rod appeared during the early stages of development, at a point corresponding to the future symphysis of the lower jaw. This rod sent off a process parallel with the base of the skull, which ossified to become the pterygoid and palatine bones, and itself became divided into a short proximal and a long distal portion. The latter was named, after its discoverer, Meckel's cartilage. The proximal division in the mammal ossified and lost its connection

with the pterygoid, becoming a small bone called the incus. In the bird the corresponding part enlarged, ossified and became the os quadratum. Huxley concluded that it was clear that 'the os quadratum of the bird is the homologue of the incus of the mammal, and has nothing to do with the tympanic bone.'<sup>53</sup>

Huxley's more general conclusion, to which his whole argument was directed, was that so far as the cranium proper of the bird and the mammal was concerned, 'not a part exists in one which is not readily discoverable in the same position, and performing the same essential functions in the other.' Huxley had been particularly anxious to show that the petrosal, squamosal, mastoid, and articular bones of the ostrich were homologous with those of the sheep, because then the line of affinity could be extended to include all oviparous vertebrates and all mammals.<sup>54</sup> The determination of the homologues of these bones throughout the vertebrate series was, Huxley maintained, 'the Keystone of every theory of the skull — it is the point upon which all further reasoning must turn.'<sup>55</sup>

His description of the skull of the turtle — taken as the representative reptile — was shaped by this interest.<sup>56</sup> Difficulties attached to the extension of the standard nomenclature and plan of organisation that Huxley was trying to introduce to the skull of the turtle. In particular, much of its cranium remained unossified throughout life and this included areas at the base and sides of the head at which Huxley had placed many of his key homologues. Undeterred, Huxley proceeded to give the same names — basioccipital, basisphenoid and presphenoid — to portions of these areas. Their cartilaginous state became a resource to explain away any anomalies — for instance in the case of a bone which he wished to identify as the homologue of the petrosal in the bird and the mammalian petromastoid, but which did not fully match the relations of that bone in higher Vertebrates. Other anatomists, Huxley admitted, had classified this bone as an alisphenoid,<sup>57</sup> but to concede this would, he claimed, 'throw the Theory of the Skull into a state of hopeless confusion, and render a consistent terminology impossible'; that is, such a classification would subvert the interest that Huxley was striving to further. Where then was the alisphenoid? Huxley replied that it was unossified, and identified it with a section of the cartilaginous side-wall of the turtle skull, which, he asserted, had the same relations to the parietal, petrosal and basisphenoid as the alisphenoid had in the bird.<sup>58</sup>

The skulls of fishes posed Huxley with still greater difficulties. He chose as his example the

cranium of the carp — 'as it departs far less widely from the common plan, and therefore forms a better type for comparison with the skulls of other **Vertebrata** than that of any acanthopterygian or ganoid fish.'<sup>59</sup> This question-begging criterion is explained on the assumption that Huxley was actively choosing between the mass of available example that which served his goal best. Even so, it took much delicate adjustment to make the carp fit the model that Huxley had posited for the other Vertebrates.

Huxley located a bone in the carp which he wished to regard as homologous with the petrosal discerned in other types: he stressed its relational affinities with the petrosal structures of mammals, birds and reptiles. But in one major respect the homology appeared to break down. This 'petrosal' did not unite with the supraoccipital in the skull of the carp, as did its putative homologues in other oviparous animals. The carp's supraoccipital was relatively small, and the two posterior internal angles of the skull were formed by another bone which did articulate with the 'petrosal'. This bone Cuvier had called the 'occipital extreme' and had supposed it to be the homologue of the bone in the turtle that Huxley had identified as the mastoid.<sup>60</sup>

Huxley could not concur with Cuvier's suggestion. He had already criticised Cuvier's identification of a mastoid equivalent in the turtle, because his own candidate for the post, a bone connected with the posterior edge of the petrosal (via cartilage) and with exoccipital, supraoccipital and basioccipital, possessed the same relations as the mastoid in the bird and sheep.<sup>61</sup> Moreover, he had also found a structure in the carp that answered to his requirements of a mastoid. This would appear to make Cuvier's 'occipital extreme' an anomaly: a bone with no homologue; but this, Huxley argued, was only so if 'our comparisons be confined to adult **Vertebrata**.'<sup>62</sup>

Again, embryology provided a resource to determine the 'true' homologies of this bone. Rathke had isolated three centres of ossification in the area of the auditory labyrinth. The anterior of these became the petrosal, while the posterior ossified to unite with the exoccipital to form the mastoid in the adult. The third, superior, ossification in birds and reptiles eventually coalesced with the supraoccipital. If the last ossification were to remain distinct, as the mastoid did in the turtle, then, Huxley pointed out, it would occupy the same position as the 'occipital extreme' in the fish. An intermediate form could, he argued, be discerned in the snake, where a bone called the **os epioticum** was distinct in the young animal

but coalesced with the supraoccipital in later life. The ichthyan 'occipital extreme' was, therefore, the homologue of the structures which resulted from the third centre of ossification, which had the peculiarity of remaining separate throughout life instead of joining with the supraoccipital. In this way, Huxley fitted this awkward bone into his general scheme and removed the apparent objection to his identification of the ichthyan petrosal that its failure to articulate with the supraoccipital posed. It **did** articulate with a bone which, in other vertebrate classes, eventually became part of the supraoccipital.<sup>63</sup>

At the conclusion of his analysis, Huxley enunciated six anatomical laws that he held to be true of all vertebrate skulls. The first of these related to the composition of the base of the skull; this Huxley said contained at most five bones. He added that these might anchylose to an indefinite extent; in some cases, therefore, these different bones would, to the uninformed observer, seem to form a single structure. This proviso was useful in view of the negotiation that the discernment of the presphenoid in the undifferentiated skull floor of the carp had required.<sup>64</sup>

Similarly, Huxley stipulated maximum numbers of bones for the roof and side walls of the skull, and gave names to these universal structures — again noting that some might coalesce. His fourth proposition stated the relation of the axial bones to the brain, and nerves and so attempted to form a link between the anatomy of the skeleton and of the viscera of the head. Huxley held that the pituitary gland could be regarded as 'marking the organic centre, as it were, of the skull — its relations to the axial cranial bones being the same, as far as I am aware, in all **Vertebrata**.'<sup>65</sup>

The fifth and sixth propositions also stated fixed relations between portions of the skull, the brain and the nerves and sense organs. The greater part or whole of the petrosal, for example, was held to lie behind the mesencephalon. The petrosal always lay behind the exit of the third division of the trigeminal nerve; while the attachments of the mandibular arch were never situated further forward than the posterior boundary of the exit of the trigeminal; consequently, they could not relate to any segment of the skull in front of the petrosal.<sup>66</sup>

But if it were possible to make propositions of this generality about all vertebrate skulls then, Huxley insisted, 'it is needless to seek for further evidence of their unity of plan.' That plan, as embodied in these laws, could easily be represented diagrammatically, and, Huxley added, there 'is

no harm in calling such a convenient diagram the “Archetype” of the skull’. However, he preferred ‘to avoid a word whose connotation is so fundamentally opposed to the spirit of modern science.’ A point that he expatiated upon later.<sup>67</sup>

These generalisations did not seem to apply to the lower orders of fish whose skulls were entirely cartilaginous. But even among those the adult forms showed considerable homologies with the general type in respect of the fixed relations of the major nerves to the auditory capsules, and of the anterior of the skull to the pituitary body. Moreover, a consideration of development revealed a still closer affinity between the skulls of these fish and those higher vertebrates. Huxley, following von Baer, argued that in the course of ontogeny, the heads of the higher vertebrates passed through stages which corresponded to the general features of the skulls of the lower vertebrates; in fact, ‘the adult crania of the lower **Vertebrata** are but special developments of conditions which the embryonic crania of the highest members of the subkingdom pass.’ The study of the development of the frog would, therefore, reveal homologues of the various cranial structures of the lamprey, shark and similar fish.<sup>68</sup>

Huxley laid the greatest emphasis on the morphological significance of such facts: the study of the mode in which the skull of vertebrated animals are developed, demonstrates the great truth which is foreshadowed by a careful and comprehensive examination of form which they present in their adult state; namely that they are all constructed upon one plan; that they differ, indeed, in the extent to which this plan is modified, but that all these modifications are foreshadowed in the series of conditions through which the skull of any of the higher **Vertebrate** passes.

He went on to detail some of the developmental homologies common to all vertebrates — including the generality of mesencephalic flexure behind which the notochord terminated; the similarity of structure of the primitive cartilaginous cranium; and the identical disposition of the mandibular arch and auditory capsules. Subsequent ossification obscured these homologies; embryology alone could demonstrate the full unity of vertebrate form.<sup>69</sup>

Huxley ostentatiously eschewed all speculative hypotheses that might be adduced to account for the facts that he had described, and instead adopted a posture of self-conscious empiricist disdain for such theorising. This attitude was most evident in his consideration of the second question that he had

posed at the beginning of the lecture: given that the vertebrate skull did conform to a common plan, was this design compatible with the 'vertebral theory' of the skull?

Huxley pointed out that his own theory made no use of the concept of an 'ideal vertebra', but he held that the validity of the vertebral theory could only be decisively settled by a consideration of the changes undergone by the skull and the spinal column during development. At first, these two were virtually indistinguishable. But 'the very earliest steps in histological differentiation exhibit the fundamental differences between the two.' In no vertebrate — except *Amphioxus* — had the notochord been traced through the whole of the floor of the cranial cavity; further, the process of division into somatomes (primitive vertebral segments) in all known vertebrates stopped short of the posterior boundary of the skull and no trace of such segmentation had been detected in the cranium itself. Chondrification and ossification also took diverse paths in the skull and the spine.<sup>70</sup>

Huxley concluded that it was impossible to reconcile these facts with a vertebral theory of the skull, 'except by drawing *ad libitum* upon the *Deus ex machina* of the speculator-imaginary "confluences," "connations," "irrelative repetitions," and shiftings of position'; that is, by resort to a battery of *ad hoc* devices which would compromise the constant signification of terms that Huxley was trying to attain. But there were other considerations active in Huxley's rejection of the vertebral theory. Huxley appealed for support to those 'who, like myself, are unable to see the propriety and advantage of introducing into science any ideal conception, which is other than the simplest possible generalized expression of observed facts, and who view with extreme aversion any attempt to introduce the phraseology and mode of thought of an obsolete and scholastic realism into biology.' The vertebral theory of the skull had, especially in the hands of Owen, become associated with such non-naturalistic forms of explanation and for this reason too it needed to be repudiated.<sup>71</sup>

However, Huxley also eschewed all resort to a theory of evolution. In this he differed from many of his successors who took up the homologies which Huxley had detected and interpreted them as evidence of common descent. In fact, at this time and for many years to come, Huxley appeared to identify such evolutionary morphology with the anti-naturalistic science which he abhorred. The evolutionary hypotheses attacked by von Baer in his Fifth Scholium were as 'transcendental' as the vertebral theory of the skull.<sup>72</sup> Even after his espousal of the Darwinian theory, Huxley retained this

suspicion of evolutionary embryology.

After 1859, the chief use that Huxley found for Darwinism was as a support for the broad principles of scientific naturalism in popular expositions. This attitude was evinced in an exchange of letters between him and Lyell in 1859. Lyell had pointed out the objections to the theory of transformation posed by the wide intervals between groups and by the absence of intermediate types. 'Must we not', he asked, 'suppose such groups to have come into the world by virtue of some "modus operandi" different from gradual development? ..... if so we may call such an unknown or as yet undiscovered modus operandi "creative" meaning merely that it has not yet been brought into the domain of Science.'<sup>73</sup>

In reply, Huxley acknowledged the difficulties to which Lyell had averred. However, he rejected the qualification of naturalism which Lyell had suggested. It was no more difficult, he insisted, to imagine a species deriving from another with which it had no obvious connection than to conceive its creation *ex nihilo*. But the former hypothesis had the advantage of being more conformable to the general pattern of scientific explanation. If species were, indeed, the direct results of the exercise of divine power, then all that the scientist could do was declare 'God is great'. While it was impossible to prove that any phenomenon was not the effect of some 'unknown Cause', Huxley argued that 'philosophy has progressed exactly as it has disregarded such possibilities and endeavoured to render every event, by ordinary reasoning.' Huxley also subscribed to the evolutionary hypothesis because

I view it as a powerful instrument of research — follow it out and it will lead us somewhere — while the other idea is like all modifications of 'final causes' a 'barren virgin' ..... And I would very strongly urge upon you that it is the logical development of uniformitarianism and that its adoption would harmonise the spirit of palaeontology with that of Physical Geology.<sup>74</sup>

Evolution was to be preferred, therefore, both because it conformed more closely to scientific naturalism than creationism and because it was potentially a 'powerful instrument of research'. However, it is significant that in seeking tendencies in scientific thought to which evolution could contribute, Huxley chose palaeontology and geology; he made no mention of his work in embryology. It was left to others, starting with Darwin himself, to make the connection between the science of individual

development and the theory of descent.



### iii. Darwin and the Natural System

Darwin and Huxley came from different traditions of zoological research. The former was essentially a field-naturalist who viewed questions of structure in the context of the ecological and historical relations of organisms. The latter was more a 'pure' morphologist: one who studied animal form in abstraction from the conditions of life. The contrast between the two is illustrated by the divergent research strategies that they pursued on their respective voyages of discovery. Darwin tried to correlate the forms of life with their geological and biological environments and made exhaustive collections of specimens from different areas. Huxley concentrated upon the microscopical study of marine fauna collected while at sea, with results that have already been described.

At first sight, therefore, Darwin was not primarily a morphologist. He owed more to the image of the 'scientific traveller' pioneered by Humboldt than to the theories of Geoffroy St Hilaire or von Baer. However, after his return to Britain Darwin made extensive studies of the form and relations of invertebrate groups like the Cirripedes. He also paid close attention to others' writings on morphology, including Huxley's works in the early 1850s. Darwin deployed this knowledge in the *Origin of Species*; he regarded the evidence of embryology as especially serviceable to his argument. As he told Asa Gray in 1860, development provided 'by far the strongest single class of facts in favour of the change of forms.'<sup>75</sup>

In particular, Darwin was interested in the concepts of 'homology' and of the 'archetype', and in the way these had been integrated into embryological analysis. He corresponded with Huxley on these subjects during the 1850s. For example, in 1853, Darwin wrote to say that he had read Huxley's paper on the Acephalous Molluscs. He confessed ignorance about much of the subject-matter, but added:

I can see its high importance. The discovery of the type or "idea" (in **your** sense, for I detest the word as used by Owen, Agassiz and Co) of each great class, I cannot doubt is one of the very highest ends of natural history ..... Such of your researches have interested me.<sup>76</sup>

However, Darwin's reservations about Huxley's argument were as significant as his endorsements. In his 1853 paper Huxley had considered the question of 'anamorphism': that is, of whether there was sufficient affinity between the major types of animal organisation to justify the assumption that one might have evolved from another. He had concluded that 'the differences between the three

[ie. vertebrate, mollusc, and articulate] archetypes are so sharp and marked, as to allow of no real transition between them.' Huxley thus sided with von Baer and Cuvier against the evolutionist Bonnet in insisting that 'Nature here leaves a magnificent hiatus among her productions'.<sup>77</sup>

In this Huxley was stressing his 'logical' view of the archetype: its status as a purely mental construct. The archetype was devised to epitomise the characteristics which distinguished groups; it should therefore be a clear and distinct concept which did not overlap with that which encompassed other groups. But if the archetypes were so separate, then so must the subordinate types which they contained; as Huxley wrote in his 'Notes on Classification',

If the types can be fixedly defined — if there is in all cases a fixed and clear distinction between Archetypes, then it is an obvious conclusion that no subordinate form of any group can no more approach the subordinate form of any other group — than the types of two groups approach one another ..... If it be true that Types are defined then it is incorrect to say that groups pass into one another by any individual forms.<sup>78</sup>

Darwin refused to accept this critique of anamorphism because of its anti-evolutionary implications. His conception of the archetype was much more flexible than Huxley's rigid categories. According to Darwin, not only was the archetype inferred from embryological evidence, but 'the archetype in imagination [is] ..... always in some degree embryonic, and therefore capable of and generally undergoing further development'.<sup>79</sup>

Huxley later revised his own views, admitting that the distinction between the main animal classes might not be as absolute as he had suggested.<sup>80</sup> Nonetheless, the contrast between Darwin and Huxley's treatment of the archetypes indicates the different interests they had in the concept. For Huxley the archetype was a device whereby actual forms could be arranged and compartmentalised; although an inference from developmental events, it was a timeless notion. For Darwin, the archetype was something with a potential for change; as such it could give an insight into the **origins** of forms.

Darwin was especially concerned with the use made of the archetype and of developmental criteria generally in classification. In his own work on the Cirripedes, he had separated this group so completely from the other Crustacea on the grounds that 'the resemblances of the larval [form] to Entomostraca is only analogical and that the natatory organs are not similar in the two, that the law

of development is very different.<sup>81</sup> This had helped him to appreciate the possibilities of assimilating current morphological practice to the theory of descent.

Darwin's assessment of the state of affairs in contemporary taxonomy and of how this might aid the acceptance of the theory of evolution appears in a series of letters he wrote to Huxley in 1857. Two main themes recurred. The first was widespread confusion about how particular groups, like the Crustacea, were to be classified; this seemed to indicate the absence of any clear idea of what classification meant. This uncertainty about the meaning of classificatory procedures was the second theme. While morphologists used notions like 'archetype' and 'homology' in their operations, and although they attached great significance to embryological evidence, there was no accepted explanation of these terms. Huxley's side of the correspondence is lost; however, at one point he seems to have argued that a purely conventionalist view of classification was adequate. To this Darwin objected that 'most naturalists look for something further, and search for "the natural system" '. This natural system, he added, was amenable to an evolutionary interpretation.<sup>82</sup>

Darwin had made the same point still more clearly in an earlier letter. He noted that no two writers defined the natural system in the same way; this left ample scope for a new definition: 'I believe it ought, in accordance with my heterodox notions, to be simply genealogical.' Darwin anticipated that Huxley would resist this suggestion; that he would maintain that as there was no 'written pedigree', this notion of taxonomy as the discernment of genetic relations would be of little value. However, Darwin insisted that

whenever heterodoxy becomes orthodoxy, ..... it will clear away an immense amount of rubbish about the value of characters and — will make the difference between analogy and homology, clear. — The time will come I believe, ..... when we shall have very fairly true genealogical trees of each great Kingdom of nature.<sup>83</sup>

This argument recurred in an expanded form in the **Origin of Species**. There it formed part of the general strategy of the book: that is, the demonstration that on evolutionary assumptions much existing knowledge could be explained and enhanced. In this case, it was the British morphological community that Darwin was trying to convince. He argued that the hypothesis of descent could contribute to an understanding of existing procedures in taxonomy and thus provide a wide explanatory

framework for the science of form. He later claimed that nothing in the *Origin* had given him as much satisfaction as the explanation of 'the wide differences in many classes between the embryo and the adult animal, and of the close resemblance of the embryos within the same class' in evolutionary terms.<sup>84</sup>

It was generally acknowledged, Darwin argued, that the resemblances between animals enabled them to be grouped together, not in arbitrary units, but in a series of classes in which the more specialised types were preceded by more homogenous ones. Darwin claimed that he had already explained this phenomenon in his account of selection as an agent of change in nature; there he had shown that environmental pressures would tend to achieve a specialisation of originally general forms and the elimination of any intermediate forms that did not precisely fit an ecological niche and were therefore less well adapted than their rivals. The consequence would be just that progressive differentiation and particularisation of form that taxonomists had long noted.<sup>85</sup>

There was, however, Darwin claimed, no agreement about the import of the order that was evident between natural kinds. While some naturalists considered classification to be merely a means of summarising in an economical fashion the affinities that existed between living forms, others saw the task of taxonomy to involve the discernment of a 'natural system', which was more than a convenient mnemonic contrivance. The most popular explanation of the existence of this 'system' was that it 'reveals the plan of the Creator'. Although Darwin was more inclined to side with the latter against the former group — to hold that classification was something more than pragmatic arrangement — he regarded an explanation of these facts in terms of divine design as of no scientific value. Unless, he argued, 'it be specified what order in time or space, or what else is meant by the plan of the Creator, it seems to me that nothing is thus added to our knowledge.'<sup>86</sup>

Darwin did briefly consider the teleologist's case that structural affinity could be explained in terms of the similar functional roles that were forced upon animals and the comparable organic forms that they developed to meet them. But the cursory nature of his treatment indicates how effectively Owen and others had already criticised this theory. If teleology were true, then those organs which determined the place of the animal in the economy of nature would be of most classificatory relevance — and this, Darwin maintained, was simply false. As a general rule, in fact,

the less 'any of the organisation is concerned with special habits, the more important it becomes for classification.' Consequently, physiologically important members were of less classificatory significance than rudimentary ones that served no function.<sup>87</sup>

Another aspect of current taxonomy on which Darwin laid great stress was the classificatory importance that was attached to embryonic characteristics. Naturalists like Milne Edwards and Agassiz, as well as Huxley, had insisted that these characters carried more taxonomic weight than those of the adult — and this had become a general view. However, no reason had been given for this assumption.

Darwin's contention was that the theory of descent explained all 'the foregoing rules and aids and difficulties in classification.' If it were admitted that the affinities between species were the result of evolution, and that classification was therefore a description of genetic relations, then the concept of a 'natural system' could be vindicated without resort to non-naturalistic hypotheses. Moreover, on the assumption that community of descent was the 'hidden bond which naturalists have been unconsciously seeking, and not some unknown plan of creation, or the enunciation of general propositions, and the mere putting together and separation of objects more or less alike', Darwin held that not only would current morphological and taxonomic practices receive an adequate explanation, but that they would also obtain a positive stimulus. From the perspective that he recommended, Darwin predicted that 'we may hope to make sure but slow progress' in disentangling the complex relations between different forms of life.<sup>88</sup>

Darwin tried to substantiate these claims by a review of the evolutionary explanations that could be attached to the current operational concepts of morphology. Among these he isolated that of the general plan of organisation, or 'unity of type', that was supposed to persist between different kinds and the use of the term 'homologous' to describe the affinity between the parts of various species within a group. These concepts were the core of morphology and, Darwin maintained, were more fully intelligible on the evolutionary theory than on any other hypothesis.

For example the notion of **special homology** (which Darwin attributed to Geoffroy St-Hilaire) stressed the important affinities to be derived from the constant correlation of certain parts in apparently disparate organs; while the form of each part might be greatly modified, to become a hand, a fin, or a

claw, yet the relations between them remained fixed, and therefore a means of uniting types. The consequence of a recognition of such special homology was that 'the same names can be given to the homologous bones in widely different animals', and so the goal of uniform nomenclature, for which Owen and Huxley had strived, could be attained.

But how was this highly useful unity of pattern to be explained? The doctrine of 'final causes' was particularly worthless in this regard; as Owen had shown, it could only relate the structure of each plant or animal to the whim of God and could provide no general explanatory principle. On the other hand, the explanation of special homology was manifest on Darwin's theory. Selective pressures might achieve the specific modification of some organ derived from a common ancestor, and, because of the phenomenon whereby the growth of one organ had correlative effects on others, affect the structure of associated parts. But this mode of change would not tend to modify the initial pattern of relations that different kinds had in common, and, consequently, the same relative distribution between parts would be preserved through evolutionary divergence.<sup>89</sup>

This line of argument, Darwin held, led to an important conclusion. This was the recognition that, on his theory, the general form, or archetype, of which existing beings were modifications, that Owen had consigned to the mind of God and that Huxley had regarded merely as a useful imaginary construct, corresponded to the structure of some real, past organism. On the supposition 'that the ancient progenitor, the archetype as it may be called, of all mammals, had its limbs constructed on the existing general pattern, for whatever purpose they served, we can at once perceive the plain signification of the homologous construction of the limbs throughout the class.' As another example, Darwin argued that in the case of the diverse forms of insect mouths, it was only necessary to assume a common progenitor that had an upper lip, mandibles and two pairs of maxillae, and the action of natural selection would suffice to explain the subsequent evolution of these structures into a multitude of forms which yet retained an essential community.

Natural selection could also be made to explain **serial homology**. Darwin undertook to account in these terms for the fact that the bodies of Crustacea were modifications of a basic structure repeated in the different organs. It was known that primitive life-forms tended to be metameric — that is, to be segmented into a number of discrete muscular sections, pierced by the neural and alimentary canals —

and that their segments were eminently prone to vary. It could be assumed, therefore, that natural selection would have operated to perpetuate those variations favourable to the animal while retaining the fundamental resemblance to otherwise differentiated segments of the body.

Darwin noted that naturalists already used such notions of 'modification' metaphorically. An advantage of his theory was that it supplied a literal signification for such concepts: 'modification' referred to the evolutionary process whereby the jaws of the crab, for instance, 'had really been metamorphosed during a long course of descent from true legs, or from some simple appendage.' In other words, the theory of descent explained the terms to which morphologists were already committed.<sup>90</sup>

Darwin's discussion of embryology was informed by the same interest. Certain structures in the embryo were similar, but became divergent in the adult. Further, embryos of distinct animals in the same class often showed striking resemblances. In general, the embryo in the course of development tended to rise in its level of organisation and gradually to achieve the peculiar form of the adult. Darwin argued that there was no obvious reason why these should be the patterns of development; instead organs might take the distinct parental form from the moment of their emergence. The theory of descent with modifications could, however, accommodate all of these facts.<sup>91</sup>

Darwin adduced two generalisations whereby this synthesis of evolutionary and embryological thought could be achieved. The first was that variations occurred relatively late in development; the second was that at whatever point a variation first took place, it would tend to reappear at the same stage of subsequent ontogenies. As a result of the operation of these two principles, the embryos of the several descendants of a parent species would continue to resemble each other in their early stages of development; but in the later stages, the ontogenetic process would instead record the variations which brought about speciation and would become increasingly divergent between types.

When ontogeny was viewed in this way as an accurate record of the successive stages of evolution, then the classificatory weight attached to embryological structures became understandable. If, as Darwin had already claimed, the only adequate classification of living things must be genealogical, then the embryo, regarded as 'the animal in its less modified state', had immediate taxonomic importance because it reveals the 'structure of the progenitor.' Further, the embryo displayed in its successive stages the previous ancestral affinities of the adult, with each innovation added to the chain of earlier

#### iv. "Embryos and Ancestors"

[W]hoever compares the discussions in this volume with those published twenty years ago on any branch of Natural History, will see how wide and rich a field of study has been opened up through the principle of Evolution; and such fields, without the light shed on them by this principle, would for long or for ever have remained barren.<sup>94</sup>

(Charles Darwin on August Weismann's *Studies in the Theory of Descent*)

As early as 1861 one British student of development declared that 'No one can doubt but that the present discussion of the origin and properties of species, will produce a marked benefit on the study of natural history.' This proved prophetic. However, the immediate impact of Darwin's ideas was felt not in Britain but in Germany. From there, they were reflected back to Britain in the early 1870s to become the paradigm of a school of embryological research.

As E.S. Russell wrote, 'The morphology of the fifties lent itself readily to evolutionary interpretation.' Specifically, Darwin was able to interpret the key terms of homology in terms of heredity and, therefore, to identify the 'archetype' as the 'ancestor' of existing forms.<sup>95</sup> In Germany, where a school of 'typological' morphology was well-established by 1859 especially at the University of Jena, these suggestions were eagerly accepted.

The transition to an evolutionary interpretation of homologies is exemplified by the differences between the 1859 and the 1870 editions of Gegenbaur's *Grundzüge der vergleichenden Anatomie*.<sup>11</sup> While the former edition was written in terms of the old-fashioned type theory; the latter contained a complete acceptance that morphology should in future proceed within an evolutionary framework.<sup>96</sup> For Gegenbaur, the theory of descent performed a culminating role in his attempts to establish morphology as an independent science: it explained the affinities which homology described. Moreover, evolution 'imposed upon the morphologist a specific task; it created for him a set of discrete problems which, in Gegenbaur's opinion, legitimized the discipline.'<sup>97</sup> In other words, evolution provided a normative system of goals for the science.

However, German morphologists were active in adapting the Darwinian theory to their existing system of means. They accepted only those portions which fitted easily with their prior preoccupations and practices. Darwin had distinguished two kinds of evolutionary events which were recorded in



development. One was the preservation of ancestral forms; the other was the incorporation of past adaptations which had some selective advantage into the development of a species. These corresponded to the distinction between 'homology' and 'analogy' in current morphology. In part, the difference between the two kinds of characteristics had been identified as corresponding to the different concerns of morphologists and physiologists. While the latter were concerned with how form served some use, the former were content to view structure in the abstract as a modification of an archetypal plan. Similarly, in the post-Darwinian era, German evolutionary embryologists occupied themselves with the search for ancestral homologies to the virtual exclusion of the question of how the operations of natural selection were displayed in ontogeny.

The distinction between hereditary and adaptive characteristics was clearly appreciated in one of the earliest documents of this school, Fritz Müller's *Für Darwin* (1864). Müller isolated two ways in which an embryo could depart from the parental form:

Descendants ..... reach a new goal, either by deviating sooner or later whilst still on the way towards the form of their parents, or by passing along this course without deviation, but then, instead of standing still, advancing still further.<sup>98</sup>

Of the two kinds of change the latter was of the greater interest. In effect, Müller and his school went beyond the relatively modest claims that Darwin had made for the evolutionary significance of patterns of development. Wherever variation had occurred by the addition of a new stage to the end of the existing series, they argued that the entire ancestral history of a type was preserved. This history might be obscured by special adaptations subsequently and the sequence would certainly be condensed with time. Nonetheless, Müller insisted, 'so far as the production of a species depends upon this second mode of progress, the historical development of the species will be mirrored in its developmental history.'<sup>99</sup>

Given this assumption, it was possible to do more than detect the affinities between groups on the basis of similarities in the early stages of their development. A complete family tree could be discovered through the study of the ontogeny of a species. In Weismann's words, 'the development of the individual presents the ancestral history in *nuce*, the ontogeny being a condensed recapitulation of the phylogeny.'<sup>100</sup> The German 'Darwinists' tried to make this theoretical possibility into a viable line of research in the 1870s and 1880s.

Ernst Haeckel was preeminent in this attempt. He found in the development of individuals the successive stages of phylogenetic ancestry and constructed detailed genealogies on this basis. Others were more cautious. However, the prevalent application of evolutionary notions in embryology during this period remained the search for phylogenetic patterns to the virtual exclusion of other goals. In the process, the distinctively Darwinian contribution to evolutionary thought — the theory of evolution **by natural selection** — tended to be ignored or allowed only a subordinate interest. In the eyes of the 'pure' morphologist, the propensity of embryos towards adaptive modifications was a nuisance which obscured their ancestry, rather than itself an important area of enquiry.<sup>101</sup>

When in the 1870s British scientists tried to attach the theory of evolution to a form of biological practice, the same pattern recurred. During this decade, Huxley ceased to be a leader of embryological thought precisely because he resisted this trend. He made his attitude clear in an 1874 address to the Linnean Society. There he identified Darwin, along with Cuvier and von Baer, as one of the foremost contributors to the resources of taxonomy. If a species were, like the individual, the result of development, then 'the character of that process must be taken into account when we attempt to determine its likeness or unlikeness to other species; and phylogeny, or the history of the evolution of the species, becomes no less important an element than Embryogeny in the determination of the systematic place of an animal.' But, although he conceded a genealogical classification as a theoretical possibility, Huxley immediately denied its practicality. His reason was that evolutionary connections could only be firmly based on **fossil** evidence, and that this, save in a few cases like the horse, was lacking.<sup>102</sup>

Throughout the 1870s Huxley argued that embryology alone could not be a sufficient base for phylogeny: palaeontological evidence must be the primary resource. In the absence of adequate fossils, the project of genealogical classification must be postponed.<sup>103</sup>

In this insistence Huxley showed his age. He revealed himself as one of a last generation of scientific generalists with a competence in geology and palaeontology as well as in biology. In contrast, the rising biologists of the 1860s and 1870s were much more specialists. While Huxley had held palaeontology to be the chief source of phylogenetic information and ontogeny to be a somewhat untrustworthy auxiliary, this new breed argued that embryological evidence alone could reveal the

ancestry of species. Fossils provided a separate source of information which might confirm inferences from development; however, the latter were independent of the former. Ironically, many of those who took this position were among Huxley's own protégés.

One of the earliest of these was William Kitchen Parker (1823 - 90) who in 1873 became Hunterian Professor of Comparative Anatomy at the Royal College of Surgeons. Parker held that his work was a development of the lines of research intimated by Huxley's 'On the Vertebrate Skull'; in particular, he undertook to expand upon the unity of plan which Huxley had held to obtain between the vertebrates. During the 1860s and early 1870s, Parker laboriously described the processes of cranial development in each of the major vertebrate groups in order to put Huxley's doctrines beyond dispute. So impressive were his meticulous observations that as late as 1937 Gavin De Beer wrote that these monographs 'represent a landmark in the comparative study of the development of the homologous structures in a large number of different forms.'<sup>104</sup>

In 1877, Parker, in conjunction with G.T. Bettany, a fellow of Caius College Cambridge, brought together these studies as a 'sketch of the history of the skull in the principle types of vertebrates', which set out to 'narrate the facts by means of a constant terminology, amplifying what Prof. Huxley has admirably developed.'<sup>105</sup> To this extent, Huxley's stated goal in his Croonian lecture of laying the groundwork of a programme of morphology by establishing a set of basic concepts had been achieved.

However, both in this book and in his previous papers, Parker went beyond Huxley in arguing for an evolutionary understanding of the homologies of vertebrates. He maintained that an evolutionary perspective greatly enhanced the appeal of embryology as well as explaining the unity of plan displayed by organisms. From this vantage-point, new orientations for research and speculation opened up: in particular, the theory of descent invited reflection upon the 'empty spaces in the great vertebrate circle'. These gaps in phylogenetic knowledge could be filled by the study of embryological evidence which circumvented the deficiencies in the fossil record. For example, the development of the frog revealed a close affinity between certain stages of amphibian ontogeny and the adult structure of the lamprey. The implication was that both had derived from closely-related ancestors.<sup>106</sup>

Parker had been still more enthusiastic about these possibilities in his earlier discussion of the fowl. There he had described the isomorphisms of the skull of the bird as a **palimpsest** from which

several layers of writing were recoverable. Having 'erased' the characters of the culminatory form, Parker found himself

among the sombre Grouse; and then, towards incultation, the characters of the Sandgrouse and Hemipod stood out before me. Rubbing these away in my downward work the form of the Tinamom looked me in the face; then the aberrant Ostrich seemed to be described in large archaic characters; a little while, and these faded into what could just be read off as pertaining to the Sea-Turtle; whilst underlying the whole, the Fish in its simplest Myxinoid form could be traced in morphological hieroglyphics.<sup>107</sup>

The book which Parker produced with Bettany was intended mainly as an aid to students of embryology; theoretical discussion was largely eschewed. But the authors did emphasise the context in which they wished the facts recorded to be viewed. They presented their work as part of the 'transition from the darkness of archetypal fancies to the clear light of verifiable history.' After summarising the major homologies of vertebrate skulls, they enquired into 'the import of these things' and into 'their place in our conception of Nature'. Their conclusion was that the processes of growth and differentiation revealed not only 'how the individual is built up, but in addition link it to its fellow creatures'. The embryo was 'not for the sake of the individual alone; it expresses a condensed history, a manifest relationship.' In its transformations, the embryo mirrored the secular changes and, therefore, the genealogical relations of organic nature.<sup>108</sup>

Such arguments were based upon a facile translation of the relations of homology into tokens of descent. While Parker's own efforts, although respected, inspired few imitators, another close associate of Huxley, E. Ray Lankester, did more to generalise this transition from a typological to an evolutionary model in morphology. Lankester was also an important figure in the propagation of the principles of German embryology in Britain. He performed these roles in two ways: as an author and as an editor.

Firstly, in his own writings between 1870 and 1873, Lankester tried to mediate between the current and an evolutionary understanding of such terms as homology. In the absence of a theory of evolution, Lankester argued, homologies had been referred by Owen and his kind to an 'ideal type' corresponding to each group of organisms. Morphologists like Huxley had rejected the transcendental version of the doctrine, but had supplied no adequate alternative explanation of homology. That

explanation was to be found in the Darwinian theory.<sup>109</sup> It was necessary, Lankester maintained, to modify the main concepts of morphology to make them more amenable to evolutionary interpretation.

'Homology' was still used to describe all structural resemblances, but Lankester maintained that it was necessary to distinguish conceptually those affinities that were the result of descent from those that were the outcome of other influences. The evolutionary sense of homology, as Lankester defined it, was that 'by asserting an organ A in an animal  $\alpha$  to be homologous with an organ B in an animal  $\beta$ ', the morphologist meant that 'in some common ancestor  $\mathcal{H}$  the organs A and B were represented by an organ C, and that  $\alpha$  and  $\beta$  have inherited their organs A and B from  $\mathcal{H}$ .' Such resemblances Lankester described as **homogenetic**.

But not all structural resemblances among types could be explained in this way. For instance, the four cavities of the bird's heart were generally regarded as homologous with the four cavities of the mammal; but since the common ancestor of both classes probably had only three cavities, this affinity could not be one of homogeny. An alternative form of explanation for such structures was needed and Lankester found it in a neo-Lamarckian theory of heredity: animals confronted with the same environmental challenges developed similar organic contrivances which eventually became hereditary. Lankester noted the proximity of this definition to that of those likenesses currently subsumed under the term 'affinity', but argued that the 'homoplastic' agreements that he had identified involved a much stronger degree of resemblance than those included in the older term.<sup>111</sup>

The distinction between 'homogenetic' and 'homoplastic' affinities mirrored that between 'ancestral' and 'adaptive' characters. Lankester's argument illustrates the fashion in which established morphological terms were being renegotiated in the 1870s to bring them into line with the presumptions of evolution. The sense in which British morphologists used 'homology' in this and the next decade tended to conform closely to the notion of homogenetic relation that Lankester had put forward. Structural affinities, especially those found in the embryo, were held to correspond to episodes in evolutionary history, and on this basis vast edifices of phylogenetic speculation were built. Lankester himself set this trend in 1872 in a series of lectures on zoological classification that he gave at the University Museum in Oxford, the substance of which was published in the following year.

At the outset of these he declared that a

Natural classification in modern zoology — in the zoology which recognizes in the various forms of living things the expression of one part of the general result proceeding from the continuous operation of physical forces — is a genealogical tree.

Therefore the questions that the taxonomist needed to ask of any organisms were 'Have we any grounds for believing this lot of forms to have a common ancestry with that lot? Which of these, again, give evidence of closer kinship? and which represent divergent lines of descent?'

The chief means to answer these questions and to determine genealogy lay, Lankester maintained, in the fact that the individual animals living at the present day, in the process of reproduction, revert to the original simple condition (or nearly so) from which they have in the course of long ages been evolved as specific forms.

Lankester went on to give an explicit statement of the 'theory of recapitulation' that underlay these procedures. This supposed that every step in the process of development corresponded to an ancestral form through which the organism had passed: 'in fact, the development of the individual is an epitome of the development of the species'. Huxley had conceded much the same, in principle; but he had maintained that later adaptations obscured the evolutionary history that had been imprinted upon ontogeny to such a degree that its discernment was impossible. Lankester also admitted that the recovery of the phylogenetic record was, for this reason, problematic. However, he held that the interference of secondary factors could be eliminated by a judicious use of embryological reasoning, and that 'in proportion as this can be effected we have in our hands in the recapitulation hypothesis the means of determining the pedigree of all organisms.'<sup>112</sup> In particular, embryology could cast light on invertebrate evolution, a field to which palaeontology had little to contribute.

Lankester maintained that it was only in the last five to ten years that invertebrate development had been intensively studied — which was hardly fair to Huxley's earlier efforts. But Lankester was right to point out that it was only in this period that new techniques to harden the egg, embed it in a matrix and then cut it into sections had been devised by Russian scientists. These greatly widened the scope for the study of invertebrate ontogeny; Lankester proposed to follow the lead of Fritz Müller, Weismann, Kowalewsky and Haeckel in this field, and to continue the investigation within an evolutionary frame of reference. Thereby, he argued, 'we may ..... draw conclusions of the greatest importance for genealogical

classification.'<sup>113</sup>

Despite his previous diatribes against 'Platonic' morphology, Lankester recognised the extent to which this method of ordering animals resembled that of Geoffroy, Owen and the *Naturphilosophen*. He remarked that the Cuvierian distinction between four discrete types of animal form was of little use in constructing a genealogical classification because it referred merely to four modes of functional adaptation. On the other hand, the 'doctrine of the unity type', with its notion of diverse forms united by their derivation from a common archetype, 'seems to me in closer agreement with facts made known by recent embryology.' It was also capable of extrapolation into an evolutionary context. This was the position that Gegenbaur had taken in his *Comparative anatomy* (1870), where invertebrates were grouped as adaptations of an original form. Lankester approved this procedure, both because of its consonance with other morphological assumptions, and because it 'seems likely to lead into the most productive lines of research.'<sup>114</sup>

With this pragmatic end in sight, Lankester set out to describe processes of development that might be regarded as 'typical'; that is, which arose from general structures and which pursued homologous paths in a variety of cases. In part, the criteria that he devised were only elaborations of Huxley's concepts; of special importance among these was the common development of all the Metazoa from ectodermal and endodermal layers.<sup>115</sup> But Lankester also placed great weight on the significance of other structures, notably the circulatory system, as a guide to genealogical relation. A true blood-lymph-system, he argued, was only found in those animals in which a third-'mesoblastic'-layer was formed between the other two. Thus a broad distinction was possible on these grounds between the more primitive 'Dipoblastica', which developed from two layers, and the 'Tripoblastica' which developed from three.

The latter group included Vermes, Echinoderms, Molluscs, Vertebrates and Arthropods, and it was possible to delineate the evolutionary relations between these classes on the basis of the degree of complexity that their circulation displayed. The simplest form of lymph-system appeared in the flat-worms; the rudimentary vascular structures that appeared in various genera of leech provided a 'bridge' between these simpler animals and the higher lymphatic and vascular organisations. All of the Tripoblastica could, Lankester maintained, be arranged in this manner along a single series, and all could be regarded

as possessing a modified version of an archetypal, ancestral, primary blood-lymph-cavity.<sup>116</sup>

Not only did the Tripoblastica exhibit this unity of type and gradual differentiation in respect of their main viscera, but also in other areas of the body. For instance, Lankester held the 'prostomium' – or region in front of the mouth in relation to which the sensory ganglia grew – to be 'homogenous throughout the series'. This structure, in other words, was derived from the Tripoblastic ancestor. Further genealogical affinities appeared when it was recognised that certain Dipoblastica, such as the Actinians, possessed a rudimentary prostomium; these, it could be inferred, resembled the transitional form between the two patterns of development.<sup>117</sup>

The recognition of such 'homogenies' was, Lankester argued, an incentive to further research designed to reveal the affinities between other parts and the probable course of the evolutionary process that had constituted a particular class. In 1877 Lankester published the results of his own investigations in this field; he presented his 'Planula' theory as an account of the earliest stages of animal evolution. In his 1873 paper, Lankester had given the name Planula to a hollow polyblast lined by two layers of cells. This being bore obvious resemblances to Haeckel's 'gastrula', and it was a token of how far phylogeny had become a fully-fledged scientific discipline that Lankester spent much time distinguishing his view of the primitive ancestor from that of his rival.

The most important issues between Lankester and Haeckel were over the nature of the primitive orifices of the blastomere and their relations to the adult mouth and anus, and over the origin of the gastric cavity itself. Huxley in his 1876 paper on the 'Classification of the animal kingdom' had taken Haeckel's categories and put them to classificatory use; he had argued for a distinction between 'archaeostomatic' animals in which the original orifice persisted as the mouth and 'deuterostomatic' animals in which it closed and the mouth developed from a new opening. Lankester held that there was no evidence to show that the original orifice in the 'planula', was a mouth, and he left open the relation of this orifice to the adult apertures. Of more phylogenetic significance was the disagreement between Haeckel and Lankester about the way in which the primitive gut had formed: the former regarded an invagination of the body wall to be the most likely hypothesis; the latter held that a delamination of inner cells was more probable.<sup>118</sup>

Lankester defended this view on the grounds that delamination fitted in better with the account



which tried to present certain crucial events in ontogeny as the representations of evolutionary turning-points. He claimed that the first historical stage, the simple unicellular ancestor, was reproduced in the ovum. In the course of evolution the 'Monoblast' had divided and given rise to spherical colonies comprising many cells; this 'polyblast' corresponded to the 'Mulberry' stage of the embryo that Haeckel had called the 'Morula'. The Morula, in turn, embodied two historical steps: one where the cells were packed tightly together and the other where a cavity appeared in their midst; this was the 'blastula' of Haeckel and the 'blastocoel' of Huxley. The latter form was preserved in the blastula stage of the jellyfish, *Geryonia*.<sup>119</sup>

During this stage the gastric cavity was formed and, consequently, a distinction arose between those cells that lined the gut and those that were external. At this point, the embryo corresponded to the ancestral 'Dipoblastic Planula'; in view of the phylogenetic significance that Lankester had attached to the developments that followed the appearance of this two-layered form, from which all subsequent Metazoic organic systems had grown, the formation of the gastric cavity was an evolutionary event of the highest importance. Lankester argued that it made more sense to regard this change as the outcome of an internal differentiation of the ancestor, rather than of invagination. The former view could be referred to an adaptation which made the digestion more efficient, whereas no selective advantage could be associated with the latter development. The fact that in almost all known present-day cases, the gastric cavity was formed by invagination posed a problem; but Lankester circumvented it by arguing that this was a relatively recent accretion and of no evolutionary import.

The next stage in evolution was the appearance of a mouth in the Dipoblastula. Lankester held that this occurred by an 'inruption', rather than by 'eruption' or 'disruption' of the ectodermal cells. A similar ingrowth was preserved in the formation of the pharynx in Molluscs, Arthropods and Vermes.<sup>120</sup>

Lankester maintained that the subsequent stage in evolution — the progress from Dipoblastic to Tripoblastic organisms — was intimately connected with the formation of the secondary body-chamber, the 'coelum'. The mesoderm had developed in the ancestral series as a progressive specialisation of the lower parts of ectodermal cells — a process which still occurred in the ontogeny of the Tripoblastica. Simultaneously, the ancestral form had sent 'diverticulae' from the enteron into the body-wall; these closed off and united to form a coelum bounded by the new mesodermal layer. This development

underlay the evolution of the circulatory system, as Lankester had argued before, and this historical episode was recorded in the embryos of widely different Triploblastulae such as Echinoderms and Brachiopods.<sup>121</sup>

In discussing the evolution of external form, Lankester held that radial symmetry had preceded bilateral in animal evolution. The original Diploblastula was at first spherical with corresponding symmetry; but the development of a mouth gave rise to a structural axis and to radial symmetry. This condition was preserved in the Coelentrata. The next step was the differentiation of an upper and a lower surface of the animal in relation to its axis of movement; at the same time, a left and right side became distinguished and so bilateral symmetry was established. Neural changes accompanied this transformation; in particular, a 'prostomiate' region developed anterior to the mouth and became the terminus of the nerve chains that now ran along an axis parallel to the old gastric channel.<sup>122</sup>

In these, and in similar hypotheses as to the origins of certain organs, Lankester displayed a form of argument that was typical of contemporary phylogeny. To give an account of how a given structure might have developed from a more primitive ancestral one was alleged to be equivalent to explaining that structure. More generally, the claim that ontogeny recapitulated phylogeny was held to explain the process of development as a whole: its particular form was the result of historical contingency. But, as will be seen below, Lankester himself did not regard phylogeny as a sufficient explanation of development.

When he turned to taxonomic issues the only classificatory principles that Lankester offered were that organisms of like structure, 'are related to one another by blood with a degree of closeness which is in direct proportion to the closeness of the likeness'. The general effect of evolution, Lankester added, had been to 'effect a progress from simpler structural conditions to more complicated.' From this it could be inferred that the 'more simple organisms which today exist are surviving representatives of the earlier phases of organic evolution.' In consequence, existing organisms could be arranged, according to their complexity of structure, in several ascending series: 'the degrees in which represent so many stages attained to and passed through by the ancestors of the most highly complicated members of the series.' Those homologies that were anomalous to the classifications so produced, were

to be relegated to the class of 'homoplasms'. Another device by which Lankester accommodated awkward groups was to stress 'degeneration' as an evolutionary process whereby animals of a certain level of organisation reverted to an earlier form. In his classification, Lankester provided a category of 'appendices' to phyla in which such degenerates could be accommodated.<sup>123</sup>

Most of Lankester's argument was merely an extreme statement of current morphological practice. But, in one respect, he departed from the mainstream. This was in his recognition that even if the phenomena of individual development could be considered as 'more or less slurred and interrupted recapitulations or epitomes of the historic development', this was not an exhaustive explanation of ontogeny. It was still necessary to inquire what the immediate causes of these changes were and so to 'come nearer to the ultimate goal of biology which is accounting for the phenomena of living matter or protoplasm by reference to the laws of chemistry and physics.' To do this, it was necessary to relate structure to function: to approximate to a **physiological**, rather than a phylogenetic explanation. Lankester realised that the Darwinian theory could impinge upon such an account, in as far as the perpetuation of particular functional contrivances in the embryo could be seen to bestow some advantage in the struggle for survival. But, ultimately, it had to be acknowledged that the 'possibility of development is solely due to the physico-chemical constitution of protoplasm', and in particular to its dual capacities to vary and to retain in its hereditary 'memory' the pattern of these variations.<sup>124</sup>

The particular hypotheses that Lankester put forward as possible explanations of developmental processes are mainly of negative interest; they demonstrate the difficulties of any mechanistic account of ontogeny in the absence of an established theory of heredity and of some notion of the chemical agents at work in the ovum and blastomere. The best that Lankester could offer to explain the basic process of cellular differentiation was to ascribe it to 'the peculiar conditions of molecular cohesion in protoplasm'.<sup>125</sup> Crude as these notions were, however, they presaged the major shift in embryological thinking that occurred in the 1890s.

In 1872 Lankester took over from his father as editor of the **Quarterly Journal of Microscopical Science**. Previously, this periodical had given roughly equal coverage to topics like histology and microscopic technique as well as to articles of development. Under the younger Lankester's control,

the **QJMS** became increasingly a journal of embryology. Moreover, Lankester undertook to direct British embryology onto a particular path, that which had been laid by the Germans.

He pursued this policy by publishing in translation the results of Continental embryologists working within the evolutionary paradigm. For instance, Haeckel's 'Gastraea Theory' appeared in the **QJMS** for 1874. Lankester himself produced a discussion of the leading doctrines of Haeckel's paper, laying special stress upon the distinction between 'palingeny' (embryonic structures attributable to heredity) and 'cenogeny' (those derived from adaptation). Lankester also noted the ontogenetic mechanisms Haeckel had proposed to explain evolutionary change, such as 'heterochrony' (the occurrence of features out of their previous sequence) and 'heterotropy' (the appearance of structures out of their previous place).<sup>126</sup>

In addition to importing such theoretical resources, Lankester kept close watch on technical and institutional developments in Continental embryology. In 1876 he noted the work of Bobretzky at Dohrn's zoological station in Naples. Lankester especially emphasised the techniques which Bobretzky had developed for hardening and slicing embryos and their potentials for the study of invertebrate ontogeny. The example of such workers, who combined high empirical skills with an awareness of the evolutionary import of their researches, would, Lankester hoped, convince young scientists in Britain of the vast opportunities inherent in embryology.<sup>127</sup>

Lankester aided those British biologists who heeded this call by publishing their work in the **QJMS**; notably, Francis Balfour's early embryological papers appeared in this journal. The doctrines they intimated were later to be developed into the fullest statement of 'Darwinian' embryology in Britain.<sup>128</sup> They were also the leading product of the school of embryology which developed in Cambridge during the 1870s and 1880s under the aegis of Michael Foster.

#### v. Cambridge Embryology, 1870 - 1890

Foster's work in creating an internationally famous school of physiology at Cambridge has recently received attention.<sup>129</sup> However, his influence was not confined to this branch of life science; rather, Foster initiated vigorous activity in several areas of biology, including embryology. As a result, the 1870s saw the creation in Cambridge of a distinct body of pedagogic and research practice sustained by highly developed theoretical resources. The students who passed through this school went on to become the leading figures in British embryology in the last quarter of the nineteenth century.

Francis Balfour (1851 - 82) was foremost among them. Balfour's career was in many respects typical of other members of the school. After studying under Foster, Balfour went for a period of advanced training in embryological technique to the zoological station at Naples. Upon his return, he obtained a fellowship from Trinity College and a University post in animal morphology. In 1876 Balfour gave the first course in embryology in Cambridge; this flourished and gave rise to a number of derivative courses. In recognition of Balfour's achievement, the University awarded him a personal chair in Animal Morphology in 1882.<sup>130</sup>

In conjunction with Foster, Balfour produced in 1874 an elementary textbook of embryology in which the course of instruction was organised around the study of the developing chick.<sup>131</sup> This was complemented in 1881 by Balfour's *Treatise of Comparative Embryology* which became the accepted advanced text for a generation of students of embryology.<sup>132</sup>

Despite the evident gap between these two works, and between Balfour's basic course and the elaborate practice of Dohrn and the other Continentals, contemporaries saw these various levels of activity as closely intertwined. As one reviewer put it, the work of Haeckel in Germany and of Lankester in Britain had revealed the great scope for further embryological enquiries: their results indicated 'how large a field is opened up for the student of every branch of natural science by embryology'. But students had to be taught to walk before they could run. Consequently, Foster and Balfour's elementary textbook was a necessary precondition for the establishment of a school of advanced embryological research in Britain.<sup>133</sup>

In 1887, A.C. Haddon, a former student of Balfour and then Professor of Zoology at the Royal College of Science in Dublin, produced a further text: *An Introduction to the Study of Embryology*.

This was seen as filling the gap between the basic and the advanced expositions of Cambridge embryology then available. In E.B. Poulton's words, 'an ideal course of embryology will begin with Foster and Balfour, continue with Haddon, and end, as far as text-books are concerned, with Balfour.'<sup>134</sup> Balfour's **Comparative Embryology** was therefore seen as the most complete exposition of the theory and practice of the school.

Medical students, above all, were meant to be attracted by such doctrines and by the courses which developed them. In embryology, as in other branches of Victorian biology, the medical profession was identified as the most likely source of patronage for the discipline. However, there were difficulties in the way of this strategy. According to Lankester these consisted of the 'stereotyped curriculum' in embryology current in the medical schools; this curriculum tended to circumscribe severely the subject-matter learned by the medical student and to exclude 'impractical' aspects of the science from his education.<sup>135</sup>

Lankester held that the kind of embryology embodied in Balfour's textbook could help to break down such prejudices. Physicians had always appreciated the relevance of a knowledge of human development to gynaecology and to obstetrics, but had tended to view the embryology of other animals as irrelevant, although they needed to approach certain developmental facts through the study of chicks and similar exemplars. In consequence, a division had appeared between 'zoological' and 'medical' embryology. Lankester claimed that Balfour's book marked an 'epoch in science' when these two separate streams would join. By its demonstration of the unity of development evident throughout the animal kingdom, Balfour's work highlighted the significance of a wide variety of zoological data to the medical histologist, who could no longer be content to try to understand the processes of cleavage and of differentiation by the limited comparisons afforded by a few traditional types. Instead, 'he must make common cause with the zoologist, and embrace the whole animal kingdom in his view.' The final justification of this view was that all animals, including man, were united by common descent; in consequence if medical knowledge were to move away from its present barren path, 'the whole evolutionary series connected with man .... must be made the subject of experiment.' Through the elucidation of the homologies that proceeded from the operations of evolution, Balfour had shown that 'embryology has become one body of doctrine

equally significant for the practical ends of the medical man and for the speculative concerns of the philosopher and naturalist'; as a result, the doctrines of cell structure and of evolution, 'taken together serve to unite the interests of scattered, and sometimes reciprocally contemptuous groups of medical men.'<sup>136</sup>

Balfour's **Comparative embryology** certainly devoted great attention to the implications of evolution for the subject, though without any overt effort to stress the supposed medical relevance of these notions. Balfour seemed more concerned with the relative importance that could be attached to embryology as a branch of morphology on the evolutionary hypothesis, and with the lines of investigation that followed from it. In his introduction, Balfour gave a formal statement of these relations between evolutionary, embryological and taxonomic conceptions.

He followed contemporary practice in holding the connection between embryology and evolution to lie in the theory of heredity postulated by the latter. Balfour held that homologies which occurred within the animal series were a 'special case of the law of heredity'; when interpreted in line with the theory of descent, this law became equivalent to the principle of recapitulation, asserting that 'each organism in the course of its individual ontogeny repeats the history of its ancestral development.'

At the same time, this recapitulation illustrated the other main aspect of evolutionary thought – variation – and its connections with heredity. Each organism reproduced 'the variations inherited from all its ancestors at successive stages in its individual ontogeny, which correspond with those at which the variations appeared in ancestors.' Recapitulation, therefore, was a necessary consequence of the combined action of heredity and variation through evolutionary time. In consequence of these facts, 'Comparative Embryology has important bearings on phylogeny or the history of the race or group, which constitutes one of the most important branches of Zoology.'<sup>137</sup>

The evolutionary theory was therefore central to Balfour's conception of embryology: by aiming at the construction of a phylogeny, the student of development was enabled 'to construct a science out of the rough mass of facts' and to avoid the trap of simply accumulating disconnected observations. Balfour detailed some of the specific research goals that issued from a phylogenetic orientation: these included the discovery of the ancestral forms common to the Metazoa; the determination of the extent to which any given larval form recurred in the ontogeny of the members

of one or more group and whether this could be interpreted as an ancestor of these groups; the correlation of the embryonic with the fossil record; the identification of rudimentary organs in one animal that were functional in another; and the discernment of transitory structures in the development of some animal that were permanent in some lower form.

These latter considerations extended the bounds of phylogeny to include the study of the way in which particular organs developed; this was the other major field of embryology that Balfour had isolated. Within the area of organology further problems were suggested for the phylogenist, such as enquiry into the origins of the germinal layers and of the primary epithelial, nervous and muscular tissues, and their mutual relations. The origin of organs from particular layers and the gradual development of more from less complicated structures provided more potentially fruitful focii of research. Again, it is important to note that in this discussion, Balfour meant by an 'explanation' of such structures a **history** of their evolutionary origins and transmutations.<sup>138</sup>

Balfour's own researches within the phylogenetic paradigm may be illustrated by three examples: namely, his consideration of the phylogeny of the Crustacea; of the ancestral form of the Chordata (a topic of persistent interest to contemporary morphologists)<sup>139</sup>; and by his account of archetypal larval forms.

Balfour noted that Fritz Muller had identified the Nauplius as the closest living representative of the Crustacean ancestor. The Nauplius was a larval form of certain Crustaceae, characterised by having three pairs of appendages — two 'antennae', one of which served a sensory and the other a motor function — and one pair of mandibles which lacked a cutting edge; the body was unsegmented and there was a single median eye; the second pair of antennae were innervated by a sub-oesophageal ganglion.<sup>140</sup>

These characteristics, Balfour asserted, were only 'capable of being explained phylogenetically'. A larval form which possessed at least some Nauplian characters appeared in the ontogeny of all major Crustacean groups; this, he argued, supported the conclusion that the Nauplius was a copy of the ancestor from which the phylum had sprung. The existing Nauplius was structurally inadequate for this role, however, and Balfour supplemented its anatomical resources by adding a segmented body, a heart and a cephalo-thoracic shield to the structure of the ancestor. His confidence in being able to infer the nature of the ancestor from the form of its progeny extended to surmising that its 'tail ended in a fork



between the prongs of which the anus opened; and the mouth was protected by a large upper lip.' In fact, the Crustacean archetype probably most resembled an 'Apus larva at the moult immediately before the appendages lose their Nauplius character.'

Balfour next considered the relations that could be inferred between this ancestor and various forms of living Crustacea: were they descended directly from the Nauplius, or were they branches from some central stem? Balfour held the phyllopoda (one class of Brachiopod) to be the survivors of a group that was central to Crustacean evolution because it most closely resembled the Nauplius ancestor. He next tried to locate the Malacostraca (Crustacea in which the body was divided into distinct segments and the appendages sharply differentiated) in this putative pedigree. The 'best' Malacostraca, that is those which fitted Balfour's scheme most easily, passed through a Nauplius stage with the usual characteristics. In their subsequent ontogeny the Malacostraca were so homologous with the phyllopoda that Balfour concluded that they were both descended from a protophyllopod type, some of whose features were preserved in the embryos of both classes. Other classes were similarly distributed either as early separate offshoots of the main Crustacean stem (Coepoda and Ostracoda), or as derivatives of some early bivalve phyllopod form (Cirripedia).<sup>141</sup>

In considering the likely ancestral form of the Chordata, Balfour held the fundamental issue of concern to be the gradual differentiation of the head from the trunk, and, specifically, the extent to which the skull of the Chordate ancestor corresponded to, and in what way it differed from, the cranial structures of more primitive animals. In certain invertebrates, like the Arthropods and Chaetopods, the head possessed a distinct element known as the procephalic or praeoral lobe; from the epiblast covering this lobe the optic organs and the suboesophageal ganglion grew. In the Chordates there was a homologous structure, namely, 'the part containing the cerebral hemispheres and the thalamencephalon'; this part of the cranium was similarly associated with the organs of sight. The evolution of the Chordate head needed, therefore, to be regarded as a transition between these two structures, with the 'ancestor' occupying an intermediate position. Balfour argued that there was sufficient evidence to show that the posterior of the head was not separate from the trunk in the ancestral-Chordate, but had become so only in later evolution.

Balfour added further detail to his conception of the primitive Chordate by a closer study of

development. This revealed how the central nervous system had been formed primaevally: 'a groove having appeared in the ancestor of the Chordata along the median dorsal line, which caused the sides of the nerve plate ..... to be bent upwards' — just as the neural canal was formed in contemporary ontogeny. On embryological evidence also, Balfour concluded that the mouth of the ancestor was 'definitely suctorial in character, and was placed on the ventral surface immediately behind the praeoral lobes'; this mouth became modified, 'for biting purposes', and was carried to the front of the head.<sup>142</sup>

After further discussion along these lines, Balfour felt competent to pronounce on the general form of the Chordate ancestors. They must have had '(1) a notochord as their sole axial skeleton, (2) a ventral mouth, surrounded by suctorial structures, and (3) very numerous gill-slits.' Two 'degenerate' offshoots of this stock survived in *Amphioxus* (Cephalochorda) and the Ascidians (Urochorda). The direct descendants of the Protochorda — the Protovertebrata — had perished. They had been followed by 'Proto-gnathostomata', which had been succeeded by the hypothetical 'Proto-ganoidei' of which existing ganoids were descendants, some of them retaining in larval form the suctorial mouths of their ancestors.<sup>143</sup>

Balfour had, in the course of his narrative, expressed the view that larval structure was more likely, in most cases, to preserve ancestral history than the foetus. This was contrary to the usual view that larvae, because they were confronted with the imperatives of survival at an early age, would tend to accumulate secondary adaptive advantages which would reduce their value as phylogenetic evidence. Balfour nonetheless argued that certain larval forms retained the character of the ancestors of the major classes of invertebrate, and that the multiplicity of existing larvae supplied proof of the evolutionary relations that existed between these.<sup>144</sup>

Balfour distinguished six phylogenetically crucial larval types. Of these the 'Pilidium', characterised by a mouth at the centre of its ventral surface and by the absence of an anus, was the simplest and therefore the closest approximation to the ancestral form from which the others had derived. Balfour represented the major anatomical features of this 'archetype' diagrammatically, together with some of its modifications that might have been ancestral to two other of his groups of larvae.

The fact that all six groups which he had enumerated could be reduced to this common type seemed 'to indicate that all the higher groups are descended from a single stem.' Because each of the larvae formed part of the development of some major group of invertebrate, it also followed that these too could be ordered genealogically. The Rotifera, Mollusca, Chaetopoda, Gephyrea (a phylum of protostomatic coelomates now regarded as three distinct groups), and Polyzoa, all of whom grew from a 'Trochosphere' larva, were, Balfour argued, the progeny of a single ancestor; it was also likely that these shared a more remote ancestor with the flatworms. Their ancestor, and that of the Brachiopoda had possessed bilateral symmetry, and was therefore a relatively late type; the Echinoderms, on the other hand, were directly descended from the radial proto-larva, or from some close relative, which somewhat resembled the modern Medusae.<sup>145</sup>

In this, the most important text of late Victorian embryology, therefore, the same kind of evolutionary explanation as was common in Germany was adopted. The discernment of hereditary characteristics in the embryo; the identification of ancestors; and the postulation of genealogies were the highest aims of embryological practice. **Adaptive** characteristics were mentioned only in as far as they obscured aspects of phylogeny.

There were some exceptions to this general pattern. John Lubbock in 1874 had tried to explain the peculiarities of certain insect larvae in terms of adaptation to their conditions of life.<sup>146</sup> Even Lubbock, however, admitted the priority of the search for an archetypal ancestor of insects and for their genetic relations to other Arthropods among the goals of morphology. He too speculated upon what the form of the insect ancestor might have been and on how it was related to the Crustacea.<sup>147</sup>

So complete was the triumph of this mode of analysis that in 1890 it was given the status of orthodoxy at the Leeds BAAS. The President of the Biology Section that year was Arthur Milnes Marshall (1851 - 93), another product of Foster's school who had studied at the Naples station at the same time as Balfour. From 1879 Marshall was Professor of Zoology at Owen's College Manchester. Marshall held that the connection with the theory of evolution had been a vital ingredient in the success of the new science of embryology in the last two decades; so great had become its popularity that fears had been voiced lest embryology monopolise attention at the expense of other branches of biology. It was the notion that the embryo recapitulated its phylogeny, Marshall declared, that provided the 'basis

of the science of Embryology, and ..... alone justifies the extraordinary attention this science has received.' In particular, embryology was legitimated through the taxonomic significance that the recapitulation theory gave to its findings.<sup>148</sup>

At first sight, then, the recapitulation theory and the body of scientific practice associated with it were secure in their dominance over British embryology in the late nineteenth century. In 1893, Marshall produced a massive textbook in which embryological explanation was equated with the discernment of phylogenetic relations.<sup>149</sup> In Cambridge itself, Balfour's place had been taken by Adam Sedgwick (1854 - 1913), a student of Foster's who became Reader in Comparative Morphology in 1882. Sedgwick preserved the essential emphases of the Cambridge school and 'many of his students became distinguished teachers and investigators.'<sup>150</sup>

However, Sedgwick was also the author of a critique of the central dogma of Balfour's kind of embryology. He argued in 1893 that the recapitulation theory was mistaken in assuming that ontogeny **in general** preserved the sequence of evolutionary change, although particular incidents had been obscured by subsequent functional adaptations. The opposite emphasis was appropriate. While **particular** features of the embryonic series recorded ancestral characteristics, in general, 'secondary' adaptations dominated the process of development. Such variations, Sedgwick argued, 'do not merely effect the not-early period of life where they are of immediate functional importance to the animal, but, on the contrary, ..... they are inherent in the germ and affect more or less profoundly the whole development.' To suppose that the majority of ontogenetic stages could be explained in terms of heredity was, therefore, to misinterpret the causes involved.<sup>151</sup>

This was a relatively mild criticism of the recapitulation theory. Sedgwick questioned the comparative weight that should be given to heredity and adaptation in 'explaining' structure, but offered no radically new idea of what embryological explanation comprised. In contrast, during the 1890s a more thoroughgoing critique of current embryological concepts and methods became established. Not only did these critics question the previous emphasis upon phylogeny, they also denied the form of practice which had accompanied these concepts. In the place of a morphology of development, they called for a physiology of the embryo.

#### vi. The Mechanism of Development

The argument of this chapter has been that in nineteenth century Britain the use and appreciation of embryological evidence tended to reflect events in Germany. In both countries, developmental facts were put to morphological uses. Firstly von Baer and his disciple Huxley pointed out the classificatory significance of development and how an embryological perspective could give a new operational significance to such concepts as 'homology'. Then Darwin suggested how the various expedients and assumptions which derived from such morphological practice could be contained by the overarching structure of the evolutionary theory. Because of the amenability of existing assumptions to an interpretation in terms of the theory of descent, 'evolutionary' embryology dominated both Britain and Germany in the later nineteenth century.

This essentially morphological interest in embryology was reflected in the institutional contexts in which the science was pursued. In Cambridge, for instance, Balfour and Sedgwick held posts in animal morphology. Lankester, Marshall and Haddon were Professors of Zoology; however, their writings show that their brand of zoology was not of the kind which stressed the importance of field studies but a science of pure form. A similar scene was evident in Germany. Haeckel, the leader of the evolutionists, was Professor of Zoology and Comparative Anatomy at Jena from 1862 to 1919.

One result of this morphological bias was the lack of interest in the 'ecological' potential of the Darwinian theory; that is, its suggestions about how certain stages in development were suited to the conditions of life. The existence of adaptations in the development of individuals was acknowledged by such as Fritz Müller and by Lubbock, but were regarded as incidental to the main concerns of embryology. These were the discovery of ancestors and the tracing of genealogies. Still less were embryologists of this school interested in the physiology of growth or in an experimental approach to the subject.

There was between 1883 and 1884 an agitation in Britain for the creation of embryological 'laboratories'. However, the example after which Huxley, Marshall, Sedgwick, Lankester, and others, strove was the marine laboratory of the Darwinist Anton Dohrn at Naples. The proximity of this establishment to the sea gave easy access to a large supply of the marine invertebrates whose study supplied the core of evolutionary embryology. Until the late 1880s, the studies of the Naples station

were heavily weighted towards a morphological consideration of these specimens.<sup>152</sup>

In the last decade of the nineteenth century this preoccupation with form and descent began to be displaced on the Continent. Once more, the initiative came from Germany. In 1888 Wilhelm Roux (1850 - 1924) published the results of some of the first attempts at an **experimental** embryology. Roux had been a student of Haeckel, but had been more impressed by his teacher's philosophical materialism than by his phylogenetic speculations. Roux reformulated the problems of embryonic growth in terms quite different from those employed by his morphological predecessors: he asked why individual cells developed in certain ways and used experimental techniques, such as the destruction of part of the blastomere, to determine the dependence of such processes on particular material conditions. The outcome of such investigations was Roux's 'mosaic theory' of development, promulgated in the late 1880s, which linked ontogeny to the hereditary materials of the germinal cell.<sup>153</sup>

Roux's achievement, though of revolutionary significance, emerged from a distinct approach to the explanation of vital phenomena which had been current throughout the nineteenth century. This may be designated 'physiological' to distinguish it from the 'morphological' style of explanation it was to replace. Essentially, the aim of the 'physiological' investigator was to explain organic processes in terms of the action of known physical and chemical laws. The existence of such views in mid-Victorian Britain has been noted in Chapter Three above; but it was only in the last years of the century that their relevance to embryology became a prominent theme.

Wilhelm His ((1831 - 1901), one of Roux's mentors, gave an early intimation of the programme of physiological embryology to British scientists. In an open letter to the Royal Society of Edinburgh in 1888, His argued for a conceptual and methodological continuity between biology and the chemical and physical sciences. Embryology, for example, could not be studied independently of 'the general laws of matter' but must take them as a starting-point for all theories of growth and form. In place of the 'unscientific mysticism' of the evolutionists, His called for a detailed study of individual development in which each stage of ontogeny 'must be looked at as the physiological consequence of some preceding stage, and ultimately as the consequence of the act of impregnation of the egg.' Phylogeny was not excluded from the concerns of the embryologist, but its relation to ontogeny had to be reconceived. Whereas, previously, phylogeny had been held to **determine** ontogeny, now the explanation of development had to be in terms

of the physical and chemical consequences of earlier stages of development.<sup>154</sup>

D'Arcy Thompson recalled in 1917 the reception that His's views had met in Britain. They produced 'harsh criticism, and even contempt': in particular, His's doctrines were dismissed 'because such an explanation was deemed wholly inappropriate'. His's critics were the defenders of the phylogenetic school of embryology. They chose Balfour rather than Roux as their exemplar, and evolution to mechanism as their explanatory principle; they had little time for 'the mechanical or physical aspect of organic development'.<sup>155</sup>

However, between 1890 and 1918, this resistance to physiological embryology was gradually eroded and experimental embryology established in Britain at the expense of phylogeny. It is beyond the scope of this thesis to describe this transition in detail. An adequate account would have to relate this change to the various specialisms and the professional goals of the actors. It would also need to be placed in the context of the general convulsion of the life sciences in the early twentieth century. Some general points do, however, emerge from the writings of the major protagonists in these events, such as J.W. Jenkinson, the first University Lecturer in Comparative and Experimental Embryology at Oxford.

Firstly, dissatisfaction with the old embryology was expressed in terms of the inadequacy of its concepts as an explanation of ontogeny. Secondly, the model of the organism which was held sufficient to fulfil this explanatory role was mechanistic and, at least at first, reductionist. Thirdly, this perspective was intimately linked with an experimental methodology.

In effect the theory of evolution was abandoned as a theoretical resource in embryology in favour of another aspect of the cosmology of scientific naturalism: the physico-chemical theory of life. Thereby embryology was assimilated both to experimental physiology and to the rising sciences of the early twentieth century, genetics and biochemistry. Thus, at the end of this revolution, Joseph Needham, one of its most powerful advocates, concluded that T.H. Huxley had long ago supplied the philosophical foundations of the new embryology. Huxley had shown that morphology needed to be subordinated to physiology because

the anatomical aspects of animals, their external and internal forms, could be deduced  
from the interplay of physico-chemical forces within them, if we only knew enough

about these forces.<sup>156</sup>

By this time, Huxley, and the naturalist movement that he represented, had themselves become a resource for others to draw upon to suit their special interests.



## CHAPTER SIX: Naturalism and Society, 1880 - 1914

**Introduction**

The argument of the preceding chapters has been that scientific naturalism must be related to the development of a 'new' professional middle-class in Victorian Britain. The professionalisation of science was an especially relevant feature of the process. However, this did not take place in isolation; in particular, the growth of the scientific profession was intimately bound up with the transformation undergone by the medical profession in the nineteenth century. This relationship was typical of a wider community of interest which existed between many professional groups during this period.

These had tended to emerge as a group:

The new fields of professionalism — in the Civil Service, science, technology, teaching, management particularly, had emerged piecemeal with industrialization. The new professionals had initially distinguished themselves from the old professions — law, medicine, the Church — which had tended to be Tory in sympathy, long unreformed and implicated in the coils of 'Old Corruption'. But in the middle and late nineteenth century the new professions began to organize themselves, setting up institutions to defend professional standards and cultivate a professional ethos, and to claim unique expertise and even monopoly over their special fields. At the same time the old professions began to reform themselves.<sup>1</sup>

By the end of the century contemporaries had begun to recognise the appearance of the new professionals as a major feature of recent social change. Herbert Spencer naturalised the events of the preceding decades; he argued that professionalisation was part of social evolution and had close analogues in organic evolution. The germs of professional agencies arose, he argued, as 'a part of the regulative agency of society'; that is, they were initially connected with government. Later they

differentiate from it at the same time being rendered more multiform by the rise of subdivisions, severally become more coherent within themselves and more definitely marked off. The process parallels completely that by which the parts of an individual organism pass from their initial state of simplicity to their ultimate state of complexity.<sup>2</sup>

Thus the 'scientific-philosophical' class had, firstly, differentiated itself from the clergy; then philosophy had become distinguished from science. The latter development, Spencer held, was most pronounced in Germany,

that model of scientific professionalism; but even in Britain the separate identity of science had now in large part been admitted.

The next stage in the process was the **internal** differentiation of science into a large number of specialities. Each of these had its 'professors' who earned their living by that pursuit; in consequence, Spencer concluded, the 'pursuit of science ..... must be regarded as a profession.'<sup>3</sup>

It has been argued that naturalism was instrumental both to the external and to the internal definition of science. Naturalism served to demarcate an area of scientific concern and competence separate from those of philosophy and theology; thereby it legitimated the social role of the scientist as an autonomous activity. Further, specific aspects of the naturalist world-view served to constitute fields of scientific practice and to achieve the degree of inner definition in British science which Spencer noted.

Other groups made comparable use of naturalistic formulations. Alienists employed somatic theories of mind in their attempts to secure their own field of competence against attack from outsiders. This was part of a more general pattern where the parvenu medical profession sought to assert its rights against the established elites of the law and church, and to achieve a recognised power and privilege in society.

Another social use for naturalism was distinguished. Namely, its development by the ideologues of political radicalism in Victorian Britain. It is tempting to try to assimilate this movement to the growth of the new professions and to claim that radicalism was the formal political expression of this social group. Harold Perkin has, indeed, argued that the conventional identification of radicalism with the commercial and industrial bourgeoisie is misguided. The radical attitude was, <sup>نه</sup> he claims, 'natural to the professional men who formed the largest occupational group among the Radicals.'<sup>4</sup>

In any case, there is no doubt that many of the proposals of the radicals would, if implemented, have contributed to the interests of the professional middle-class. For example, the radicals called for the abolition of nepotism in government service; for competitive examinations; and for a career open to the talents. All of these are characteristics of the mode of organisation favoured by modern professions.

However, whether the radicals were or were not a movement for the professional middle-class, there is no doubt about which social group they were against. To a large extent they were a party of criticism and their polemic had one main target: the 'landed interest' in British society. Between 1815 and 1845 British radicals had sustained a bitter attack upon the dominance of the landowners in the constitution. Thereafter, 'hatred

of the landed interest has always been with the Radicals a more or less animating force.<sup>6</sup>

In the period 1846 - 1865 radicalism was in abeyance. But, with the rise of Gladstone to power, a 'fresh ebullition of Radicalism took place. The spirit of disturbance was once more let loose. One institution after another was assailed or threatened.'<sup>7</sup> Radicals like Mill and Bright were instrumental after 1865 in forcing the Second Reform Act upon a reluctant Conservative Cabinet. The Parliament that issued from the first elections under the new franchise was 'the most Radical which England had known since 1832.'<sup>8</sup>

These radicals set about attacking the power of the landed interest and of its great appanage, the Established Church, in the years that followed. They pressed for a reform of land tenure; for Church disestablishment; and for secular education. In September 1873 Joseph Chamberlain summarised the radical programme in the slogan: 'Free Church, Free Schools, Free Land, and Free Labour.'<sup>9</sup>

Just as Victorian radicals tended to have the same enemies as those of a previous era, so they employed a similar rhetoric and invoked the same kind of world-view. Radicalism, from Paine and Godwin onwards, and 'atheism' were closely connected. As part of their critique of the Church the radicals assailed its leading doctrines. Foremost among their weapons was a naturalistic account of man and the universe which undermined the leading dogmas of orthodoxy. The sciences, especially physiology, were the main source of materials for this strategy. As a result, 'men of science were regarded with some amount of suspicion and disliked by those whose instincts were conservative.'<sup>10</sup>

In fact, the social meaning of 'science' in the nineteenth century was much more complicated than this simple correlation suggests. Certain natural philosophers, like John Abernethy, Charles Bell, and William Whewell, accommodated their science to a conservative political outlook. However, their efforts were constantly matched by a tradition of naturalism which persisted throughout the nineteenth century. And through the work of such polemicists as T.C. Morgan, the connection between naturalism and radicalism was preserved.

The writings of naturalists like W.K. Clifford in the 1870s were seen as the culmination of this cultural tendency by both friend and foe. John Morley, the radical editor of the **Fortnightly Review** published many of Clifford's pieces; moreover, Morley himself was active in elaborating the social significance of these cosmological doctrines. The Conservative philosopher W.H. Mallock noted that Clifford had not been content to be a specialist teacher:

he proposed also to be a leader of men. Nor was his ambition here at all of a limited nature.'

He saw the disorder of the world, its painful perplexities of thought, and its chaos of conflicting motives, and he sought in the midst of this to inaugurate a new order, and by entirely new means. His schemes were co-extensive with the whole of human life.<sup>11</sup>

Clifford had tried to give science a moral role. A realisation of the true scheme of the universe would, he had maintained, generate a true system of interpersonal obligation and social order. The main message of his cosmology was that 'there is no God, no soul, no future life'.<sup>12</sup> Man was a purely natural being who must find his ends and means entirely within the world that science revealed. According to Clifford the universe was self-existent; matter and spirit were the dual aspects of the same substance; there was no need to assume an external power to explain any natural occurrence. There was no 'destiny or ..... providence outside of us overruling human efforts, and guiding human history to a foregone conclusion.'<sup>13</sup>

Clifford and his school therefore insisted that for moral laws 'we must look to no superhuman lawgiver':

The dim and shadowy outlines of the superhuman deity fade slowly away from before us; and as the mist of his presence floated aside we perceive with greater and greater clearness the shape of a yet grander and nobler figure — of Him who made all gods and shall unmake them.<sup>14</sup>

There could be no duties towards a transcendent God because, even if such existed, science showed that he could not influence human affairs. The contrary view that 'God orders the world' Clifford had regarded as 'an idea of the utmost danger': it represented the reactionaries' view of morality. Instead of the conventional Christian wisdom he had insisted upon the human and social results of an action as the sole criterion of morality.<sup>15</sup>

John Morley made still clearer the relation between such doctrines and a radical political stance. William Lilly, another Conservative commentator, identified Morley as the chief spokesman of the 'present-day Jacobins' who urged that it was 'upon "natural truths" ..... that the foundations of public order must rest.' This, according to Mallock, was 'the underlying rationale of Radicalism.' There was a 'logical connection' between the various aspects of the radical creed:

The negation of a God and a future life gives a harsher aspect to the darker sides of civilization, and thus creates a desire for reform that would otherwise be unimportant. By making morals relative solely to social expediency, the area of reform, or at least change, is widened, and the dignity of reform increased. By the negation of free-will, and the inclusion of human action among the subjects of science, a theoretical basis is made possible for a positive doctrine of

progress; and thus, the idea of progress being essential to the modern idea of Democracy, the philosophy of Radicalism supplies the ideal state with its ends, and the politics of Radicalism supplies it with its means.<sup>16</sup>

In other words, a naturalistic world-view supplied the 'morality' of radicalism. From this cosmology a number of normative precepts were extracted and to these the major policies of the radical party were referred. In essence, the radicals insisted on the perfectability of man; upon the necessity of reconstructing society upon 'rational' principles; and upon removing all those who clung to 'irrational' power and privilege.

Given the social implications of naturalism, reaffirmations of the existence of the soul and of the non-empirical character of morality also had a political significance. The comprehension of the professional interest of science by the wider goals of radicalism had a conservative counterpart. Clerics, like William Whewell, were regarded as spokesmen of Conservative social interests as well as of their profession. Whewell's insistence upon the transcendental foundations of knowledge, for example, was, according to J.S. Mill, an attempt to mystify the bases of institutions and so to place them above criticism.<sup>17</sup>

Other conservative polemicists, such as A.J. Balfour, responded by criticising the foundations of Mill's empiricism and the ethical naturalism that was derived from it.<sup>18</sup> As Balfour's confidant A. Seth Pringle-Pattison pointed out, while these arguments were, at first sight, contributions to the philosophy of religion and 'a demonstration of the insufficiency of Mill's empirical philosophy as a foundation for our scientific beliefs', it was no less 'a revolt against the social philosophy of the Philosophical Radicals'.<sup>19</sup>

There were continuities between the mid and late-Victorian periods in these respects. Radicalism remained a potent force, and a second generation of writers, of whom Karl Pearson was the foremost, continued to press the connections between naturalism and 'advanced' political views. However, there were also changes after 1880. Some of these were quantitative rather than qualitative: the volume of anti-naturalistic literature increased as did the urgency of these attacks. But there was also an alteration in the character of naturalistic polemic itself.

Both of these trends were the result of the 'geological shift' in British politics in the last decades of the nineteenth century. The rest of this chapter examines this disturbance and the cultural movements that followed from it.

### i. The Passing of the Old Order

Lord Salisbury set the tone for the period when he wrote to his nephew A.J. Balfour that the disastrous Conservative electoral defeat of 1880 'might be the beginning of a serious war of the classes'.<sup>20</sup> The next fifteen years were indeed marked by growing conflict in British politics and by signs of deep social divisions and discord. After a lull between 1895 and 1900 these tensions were to grow worse until the outbreak of the First World War.

The most obvious menace lay in Ireland. There long-standing social antagonisms combined with a militant nationalism to threaten rebellion and civil war. After attempts to appease the natives by piecemeal measures had failed Gladstone concluded in 1886 that the only solution was to grant home rule to Ireland. This split his own party and gave rise to an alliance between Conservatives and Liberal Unionists. The objections of the Unionists to home rule were couched in terms of loyalty to the integrity of the constitution. But much of the strength of the movement came from fears about the likely impact of submission to Irish demands on affairs at home.

To Conservative politicians like Balfour and Salisbury the nationalist rhetoric of the Irish obscured the real causes of the disturbances. These lay in the conflict between landlord and tenant. Rural violence was not in their view 'an agrarian means to a political end: it was class war'.<sup>21</sup> It was a war, moreover, which could all too easily spread to the mainland. There were similar hostilities in the British countryside and these were exacerbated by the chronic agricultural depression of the late nineteenth century. To give up the rights of the Irish landowners seemed to many Conservatives to risk similar measures in Britain.

In contrast to the Unionist position on Ireland and on land-ownership were the increasingly vociferous views of the land reformers. These came from many quarters but, most importantly, they were a growing force within the Liberal Party. Even before the home rule crisis the Party had been increasingly radical-dominated. Within the radical programme, one critic wrote in 1881, attacks on the Established Church now took second place: the primary contention of Advanced Liberals was that 'the land belongs to the people, and ought to be restored to them'.<sup>22</sup>

In itself this was a threat to the landed interest and a challenge to the Conservative Party which defended the landowners. However, the land reformers' programme had more extensive reverberations.

Attacks upon land were associated with hostility to other forms of property. They were seen as the first step towards the unearned increment taxation of Mill and trade unionist George Odger's Land Tenure Reform Association, the Single Tax of Henry George, and the land nationalization of Alfred Russell [sic] Wallace, from which the next step was short to the nationalization of railways, mines and other forms of capital, and so to full-blooded socialism.<sup>23</sup>

In effect, events in Ireland and debates about land-ownership were viewed in the context of a more general tendency in British politics: the growth of socialism.

Between the late 1850s and early 1880s there was no significant socialist movement in Britain. But as the Liberal Party moved to the left, so did socialist doctrines begin to find listeners in Britain.<sup>24</sup> In 1881 H.M. Hyndman founded the Marxist Social Democratic Federation; although initially small the influence of the SDF grew during the 1880s. The socialists found especially receptive audiences among the semi-employed labouring-class of London. Moreover, the socialist-inspired 'New Unionism' eventually infiltrated the established unions and came to dominate the Trades Union Congress by the early 1890s. The result was a more belligerent labour movement: one whose demands were no longer confined to wages and conditions of work but extended to basic questions of the distribution of wealth in society.<sup>25</sup>

Middle-class observers noted these developments with growing anxiety. St George Mivart in 1885 drew dismal comparisons between contemporary Britain and pre-revolutionary France, holding that 'the great French Revolution ..... has many an important warning for us in England.'<sup>26</sup> Others made similar uncomfortable comparisons between conditions in Britain and events in contemporary Europe; for instance, W.H. Mallock wrote in 1883 of the social conflict which

now in one place, now in another, is continually filling the air with dim rumours of revolution; which more than once has deluged Paris with blood; which keeps German cities at this moment in a state of minor siege; which embitters the conditions of civilization even when it lacks vigour to menace them; and which certain politicians ....., having done their best for a time to excite and use it in Ireland, are now endeavouring by every art in their power to make it the inspiring principle of the 'Liberalism of the future' in England.<sup>27</sup>

Because the Liberals were thus seen as the 'revolutionary party', one whose doctrines were a dangerous concession to the socialists who were ever gathering strength, the period 1880 - 90 saw a wholesale defection of many traditional Liberal supporters. Liberal policies, these alleged, were paving the way for 'a tyrannical, equalitarian democracy.'<sup>28</sup> The defectors rallied to the Conservatives thereby transforming the structure of British politics.

The 'old order', wrote R.B. Haldane in 1888, 'is passing away': The Tories were no longer the party of the landlords. The landlords still rally and will continue to rally round their standards, but only as one of a multitude of special interests which do the same from instinct, not of satisfaction or sympathy, but of self preservation.<sup>29</sup>

One feature of this struggle for survival was an attempt to controvert the teachings of socialism and to present a view of society which would conduce to stability. In part this rhetoric was directed at the working-classes. After comparing British society not 'to a house that is on fire, but to a house that is full of exceedingly inflammable materials', Mallock attributed this incendiary state of affairs to the access of the newly-educated workers to dangerous ideas. Because of general literacy, socialism had been able to spread quickly the pernicious doctrine that 'the calm readings of science correspond with the promptings of their own most dangerous passions.' What was needed was some form of mental discipline; in its absence, it was 'impossible not to see that any successful attempt to propagate in this country those explicit theories of revolution, which have already had such a fatal effect upon the Continent, might be fraught with effects hardly less fatal here, or might at all events bring us face to face with very serious social dangers.'<sup>30</sup>

Another observer drew a similar conclusion. Because of universal education socialism in Britain and in Europe had been propagated widely by books, pamphlets, and cheap newspapers. It followed that if 'Socialism is to be done away with, it must be attacked in its origin and in its means of diffusion.'<sup>31</sup>

Open repression of socialist propaganda was difficult in Britain. But the period after 1884 saw the appearance of 'a whole literature of opposition' which sought to counter the effects of socialist polemic. There was no lack of manpower to produce this literature. During the 1880s the body of British intellectuals followed the general trend and switched their political allegiance: whereas the 'intellectual and literary society of London and the Universities had been mainly Liberal; it now became mainly



Unionist.' For example, before 1885 the Fellows of Trinity College in Cambridge had been predominantly Liberal; after that date they were, as a body, Conservative.<sup>32</sup>

Between 1885 and 1900 British intellectuals, impressed by the gravity of social and political conditions produced a mass of polemic designed in some way to influence events. This literature was various: history, metaphysics and religion were drawn upon to counter the claims of radical demagogues. However, there was widespread agreement that the natural world was a central resource in this endeavour. Two main strategies can be distinguished. One was overtly anti-naturalistic; it sought to assert the dependence of the universe upon God and to show the existence of purpose within the cosmos. The other insisted upon naturalism and especially upon the need for a 'scientific' understanding of man and society. Despite their apparent antinomy these strategies were not incompatible. On the contrary, they were put to one end: the production of a profoundly conservative social and political philosophy.

## ii. The New Teleology

The 'reaction to scientific naturalism' in late Victorian Britain took many forms.<sup>33</sup> However, contemporaries discerned a pattern among the different criticisms and revisions of naturalism. Bernard Bosanquet noted in 1906 that more than one 'distinguished critic of Naturalism' had set 'Teleology against Mechanism and Epiphenomenalism'; that is, they had asserted that 'finite consciousness' had an active role in the shaping and determination of natural processes.<sup>34</sup>

This was an accurate summary of the main contention of the proponents of the new teleology. Pringle-Pattison had in 1891, upon accession to the Chair of Logic at Edinburgh, announced the 'necessity for a teleological view of the universe'. By this he did not mean a return to the search for particular adaptations to an end; rather, the new teleology

concerns itself only with the End of the whole evolution [of nature]. It concentrates itself upon the proof that there is an End, that there is an organic unity of purpose binding the whole process into one and making it intelligible ..... Every true philosophy is in this sense an attempted theodicy – the vindication of a divine purpose in things.<sup>35</sup>

James Ward similarly insisted that 'It is only in terms of mind that we can understand the unity, activity, and regularity that nature presents.'<sup>36</sup> This position was the 'antithesis of Naturalism, which regards Nature as supreme and consciousness in all its forms as the product of nature'.<sup>37</sup>

Such utterances were consonant with a widespread cultural shift in late nineteenth century Britain. This movement had three stable features. It rejected naturalism, not only as intellectually unacceptable, but as practically dangerous. It proposed as an alternative a cosmology in which nature was pervaded by divine consciousness and will and in which man's material being was subordinate to his spirituality. Finally, it asserted a particular structure for the universe and a special destiny for the human beings who inhabited it.

The repudiation of naturalism on 'moral' grounds needs to be related to the social concerns of the time. Thus in 1883 Cardinal Manning wrote of 'authority, obedience, and brotherhood' as the contraries which would disarm the socialist trinity of equality, liberty and fraternity. However, the efficacy of such values depended upon their identification as aspects of the 'natural order', not as mere human devices. Moreover, this natural order had to be endowed with a transcendental significance: its ordinances were

to be respected because 'the natural order is divine, for its author is God.' Current institutions formed part of this order and therefore shared the same divine sanction. The 'whole structure of society is pervaded by the will and power of God.'<sup>38</sup>

On such reasoning any view of the world which denied the presence of the divine will in nature also repudiated its immanence in society. In particular, any purely naturalistic account of man and the universe vitiated the social significance that could be derived from teleological interpretations of the cosmic order. This was the charge that many contemporaries brought against scientific naturalism. Moritz Kaufman in 1885 reported a connection between the spread of socialism and the loss of faith among the 'masses of working men throughout the length and breadth of the European Continent'; to a lesser extent the same alarming correlation held of 'certain classes of operatives in the large centres of [British] industry'. The materialistic tendencies of modern science had contributed to the decline of religion and to the consequent receptiveness of the working-class to false prophets: Darwin's *Origin of Species* had prepared the way for Marx's *Capital*. Kaufman concluded that nothing could 'save society from the present danger but the restitution of genuine religious belief.'<sup>39</sup>

Other critics concentrated upon the impact of the efforts of such philosophers as Mill and Spencer to put ethics on a purely naturalistic basis. These had reduced morality to a matter of calculation: to an assessment of the utility of an action to the welfare of the individual or of society. Thereby, it was alleged, they had taken the 'wonder and mystery' out of the cosmos. To leave out all reference to a 'higher' source of duty was

to analyse [man] ....., and human society by consequence, into jarring atoms with chaos for their dwelling-place. And England, which was built up to its present greatness by men who believed in Duty as a revelation, the highest they could be given, from out of the heart of Eternity, will be pulled down into the dust if, as various signs portend, a religion of agreeable sensations ..... be recognized and acted upon by the governing majority.

In particular, such a philosophy of the 'kitchen and larder' was the favoured idiom of socialists and communists who strove to bring about 'the union of labourers *versus* capitalists all the world over'.<sup>40</sup>

William Lilly maintained that this process was already far gone in France. There the 'medico-atheistic' school, the counterparts of such British naturalists as Clifford and Huxley, had eroded the

religious foundations of morality as part of its assault upon established institutions. Their morality 'derationalised' the world because 'it is fatal to the belief that reason pervades the universe.' Moreover, it was corrosive of social unity. The 'bonds of society', Lilly maintained, were ethical; if these were loosened 'fall the social system must.' It was to this end that the materialists had worked in France. They had abolished God as an authority to whom social arrangements could be referred; there was therefore no check upon human desires and the voice of the majority must prevail: 'Vox populi, Vox Dei.'<sup>41</sup>

Even writers within the utilitarian tradition had for some time voiced similar doubts about the efficacy of naturalism as a foundation of social order. Henry Sidgwick wrote in his **Methods of Ethics** that any ethical system must provide a mechanism whereby the individual could be reconciled to the sacrifice of his own interests for the 'general good'. Sidgwick had concluded that no such inducement could be supplied on merely empirical grounds: it was necessary to posit a God who had established the moral law as an integral part of his creation and had provided a set of rewards and punishments for those who obeyed or transgressed against it. Since happiness did not always follow virtue or misery evil in this life, it was necessary to assume another world in which injustices would... be remedied.<sup>42</sup>

Arthur Balfour, Sidgwick's former student, placed this abstract argument in the context of immediate social concerns in an 1888 address to the Church Congress in Manchester. The major failure of naturalism, Balfour held, was its inability to offer the promise of a future life to palliate 'all the crookedness and injustices of a crooked and unjust world.' There was a special need for such an anodyne at a time when 'sensitivity to social evils is increasing' and there was a tendency for people either to 'rush frantically to the first quack remedy' or to succumb to despair.<sup>43</sup>

A purely secular ethics might suffice to meet the needs of the bourgeoisie, 'the small and comparatively prosperous class', which had good hope of finding fulfilment on earth; but it had nothing to say to 'the more obscure multitude who are absorbed and well-nigh overwhelmed in the constant struggle with daily needs and narrow cares.' It was these, who had least to lose and most to gain from a transformation of the social system, whom Balfour wished to appease with the promise of a future life and to discipline by referring moral rules to a divine author who would judge men by their compliance

with them. A philosophy which left humanity 'divorced from all communion with God, face to face with the unthinking energies of nature' was less than useless for this task.<sup>44</sup>

Balfour elaborated these criticisms of naturalism in his popular *The Foundations of Belief* (1895). There he argued that to be effective moral systems must inspire reverence. Naturalistic ethics failed to meet this condition because they relegated morality to an insignificant status in the universe. According to naturalism, the cosmos was not inherently moral: it was not permeated by the purpose of an ethical agent. On this hypothesis moral sentiments were no more than one of the contrivances developed by the human species in the course of evolution to improve its chances of survival. When contrasted with earlier religious versions of the origins of moral rules, the inadequacy of naturalism became obvious:

Kant ..... compared the Moral Law to the starry heavens, and found them both sublime. It would, on the naturalistic hypothesis, be more appropriate to compare it to the protective blotches on the beetle's back, and to find them both ingenious. But how on this view is the 'beauty of holiness' to retain its lustre in the minds of those who know so much of its pedigree? ..... Assuredly much of the efficacy of these moral lessons will be destroyed, and the contradictions between ethical sentiment and naturalistic theory will remain obtrusive and perplexing, a constant stumbling-block to those who endeavour to combine in one harmonious creed the bare explanations of Biology and the lofty claims of Ethics.<sup>45</sup>

This was not just a theoretical problem. Balfour predicted that if naturalism ever acquired general credence it would 'at no distant date most unpleasantly translate itself into practice.'<sup>46</sup> Pringle-Pattison defended Balfour's use of this 'argument from consequences'. Balfour was fully justified to refer to the likely practical consequences of naturalism because 'no human society has ever been based upon the conclusions of materialism, and whenever this negative creed has become widely spread ..... the result has been visible in moral deterioration and social disintegration.'<sup>47</sup> Writers like Huxley might deny that their creed was materialistic; however, for their critics, the fact that they excluded the divine will from the government of the world was sufficient to earn them that epithet.

St George Mivart summarised these censures upon naturalism in his contribution to the debates that Balfour's *Foundations of Belief* aroused. Naturalism, Mivart argued, was 'absolutely destructive to every germ of morality', and, thereby, it threatened civilisation itself. In view of this,

would not a prudent reticence on the part of the philosopher be more admirable than indulging in a long course of oral incontinence without regard to consequences?<sup>48</sup>

Such diatribes had a positive complement. While naturalism threatened to contribute to the disruption of British society, a teleological cosmology was held to be a conservative agent. Evidence of the working of purposeful agency was sought in many fields: evolution was interpreted in orthogenetic terms; ontogeny was seen as the work of a quasi-spiritual entelechy; the ether was presented as the means by which God acted upon nature. An adequate account of this literature would require another thesis as long as this one. Here it is possible only to isolate the main themes of this movement: namely, that mind should be regarded as prior and superior to matter in the natural order, and that certain, socially significant structures issued from the operation of the divine will.

The form of this strategy was apparent in Balfour's work. He argued that the possibility of knowledge of the material supposed the priority of mind in the cosmos. Balfour attacked naturalism through its empiricist theory of knowledge; in its place he put the view that the mind actively interpreted experience with the aid of certain God-given concepts. An 'adequate' theory of knowledge, Balfour told Pringle-Pattison in 1886, would have to show how the 'principles which are the necessary foundations of our ordinary scientific view of the Universe are to be transcendently proved.' In this task, the philosophy of Thomas Reid was a serviceable resource.<sup>49</sup>

The solution which Balfour offered in the **Foundations of Belief** to the problem of knowledge was similar to that of the Common Sense philosophers. His essential contention was that science was a less problematic enterprise in a theistic than in an atheistic universe. Scepticism about man's ability to penetrate the phenomenal barrier that kept him from direct contact with the physical world could only be overcome on the assumption that the world was 'the work of a rational Being, who made us, in however feeble a fashion, able to understand it.' The existence of a transcendent mind, which shaped both nature and the human psyche, was therefore 'not merely tolerated, but is actually required by Science'.<sup>50</sup>

Balfour elaborated on this argument in his 1904 Presidential Address to the British Association for the Advancement of Science. There he argued that the success of modern physics in revolutionising man's vision of the universe had to be attributed to something other than mere attention to phenomena.

The modern conception of physical reality was one which experience alone could never have generated. The progressive penetration of the inner structure of matter could only be understood on the assumption that the empirical resources of science were being supplemented by a further kind of insight: the human mind was directed in its enquiries by a higher intelligence. As science proceeded, Balfour concluded, 'it leans more, not less, upon a teleological interpretation of the universe.'<sup>51</sup>

The correspondence between Balfour's epistemology and the world-view of other proponents of the new teleology was not confined to the assertion of the supervision of nature and of man's understanding of nature by a providential agent. Balfour and his like went on to insist that the determinate results of the workings of the divine will should also be stressed.

Balfour held that naturalism was but the most extreme version of a philosophy which had characterised western thought for centuries. This was 'rationalism', a creed that held experience to be the only criterion of knowledge and the individual reason to be an adequate judge of what was true and of what was moral. Balfour contended that his view of scientific knowledge showed this theory to be false. Experience could not supply basic beliefs about the world; these had to be derived from a source which was both supra-empirical and supra-individual. Ultimately, man depended upon the divine mind for knowledge; more proximately, however, the essential intuitions about nature were the property of the community and lodged in social institutions. What was true of scientific beliefs was also true of political, religious and moral ideas: the individual could not produce these in isolation, but only as part of the social whole. Society thus exercised **authority** over its members; the latter were entirely dependent on it for the bases of all thought and action.<sup>52</sup>

Nor was this subordination of 'reason' to 'authority' to be regretted. On the contrary, it was a valuable corrective to the potentially dangerous effects of rationalism:

because ..... reasoning is a force most apt to divide and disintegrate; and though division and disintegration may often be the necessary preliminary of social development, still more necessary are the forces which bind and stiffen, without which there would be no society to develop.

'Authority' did more than establish knowledge: it 'lays deep the foundations of social life; [and] ..... cements its superstructure.'<sup>53</sup>

Balfour's argument, Pringle-Pattison commented, was an answer to the 'narrow and individualistic rationalism' of the philosophical radicals.<sup>54</sup> It was an attempt to transcend the individual as the focus of epistemological, ethical and social discourse; to go beyond human ratiocinations to 'the larger or corporate reason, active in history and embodied in the social structure.'<sup>55</sup> The reply of Balfour and his kind to the irrationality of society was to point to a 'higher reason' which was realised in existing social forms. The real was the rational. Mill's prediction that 'God-given' beliefs in science would prove the easy means of sacralising 'bad institutions' was shown to be fully justified.

There are clear affinities between this tendency of the new teleology to elevate the corporate over the individual and more general patterns in European culture. Karl Mannheim described the polarisation of political and social thought which occurred after the French Revolution. On the one hand was the 'rationalism' of bourgeois radicals whose leading feature was 'a dissociation of knowledge from personalities and concrete communities'. The rationalist 'eliminates the whole context of concrete relationships in which every piece of knowledge is embedded.'<sup>56</sup>

On the other hand were those groups who stood outside the mainstream of capitalist development, especially those who continued to draw their livelihood from the land. In their characteristic cultural idiom:

'Community' is set against 'society' (to use Toennies' terminology), family against the contract, intuitive certainty against reason, spiritual against material experience.

This conservative world-view constituted an 'objective mental structure' in nineteenth century Europe. It was a resource which particular groups could employ in their attempts to formulate a 'counterlogic' to that of the rationalist.<sup>57</sup>

Balfour himself made the comparison between *naturalism* and *rationalism*; the connotation he gave to the latter term was very similar to Mannheim's usage. Throughout the nineteenth century such theories had been associated with radical bourgeois movements. The main political opposition to these had been the landowners and their auxiliaries, especially the Established clergy. This interest articulated a rival to the radical philosophy: namely, the 'Broadchurch' tradition of social thought in Victorian Britain which was

concerned to attack the atomistic and mechanistic modes of thought characteristic of



Utilitarianism and to commend an ordered and hierarchical society held together by 'natural' relations and a set of common values. These values, they argued, could best be represented and expressed by a National Church, a liberal and comprehensive institution, under the auspices of which the developments in secular thought could be synthesized with Christian dogma. Such a body would act as the repository of national traditions and aspirations and also hold in check the disintegrating social and intellectual tendencies of modern life.<sup>58</sup>

As the nineteenth century drew to a close the scope of this 'Church' grew. It was unsectarian; moreover, its priests were not necessarily clerics but were drawn from the growing lay intelligentsia.<sup>59</sup> As James Martineau wrote in 1886, there was at that time of social unrest more than ever a need for the reaffirmation of the leading tenets of conservative thought. In particular, it was necessary to draw 'all men into Divine relations' and thereby achieve 'the sanctification of human life by conscious communion with the infinitely Perfect Spirit, and the consequent enthusiasm of all pure and uniting affections.'<sup>60</sup>

Pringle-Pattison assimilated his work to a related aspect of conservative thought; namely, the attempt to vitiate the rationalist's excessive abstractness. Pringle-Pattison told J.S. Blackie in 1882 that his philosophy had one aim: 'to bring men back to the Concrete.'<sup>61</sup> In place of the analytical and individualistic bent of empiricists like Spencer and Mill, Pringle-Pattison proposed to stress by all available means the holistic character of nature and society.

These means were not confined to philosophy as such. Pringle-Pattison, like many of his contemporaries, looked to biology to substantiate this vision of reality. The 'neo-vitalism' of the period provided arguments for the existence of a teleological principle in the organism. The working of this principle fully corroborated the claim that the structure of nature was typically holistic; a functional community of unequal parts.<sup>62</sup>

The implicit social and political meaning of such utterances becomes clear when they are compared with one form of Conservative apologetic current at the time. This tended to be organicist, anti-bourgeois, and to hold that the landed-classes were the natural rulers of society. Thus, Harold G. Parsons wrote in 1900 that Britain was emerging from an era of middle-class individualism. Recent political thought showed a welcome reversion to 'the ideas of the Tudor, or organic or national period.' Toryism, he argued, stressed

the responsibilities of the different sections of society to one another. It might even be called 'socialistic, because it is founded upon the social organism'. However, Tory socialism was distinguished from the destructive socialism of the left. The Conservative worked to preserve society 'by the regulation and reformation of the body politic'; while radicals and socialists represented 'the solvent, decomposing acid' of society, Toryism 'conserves the unity and the institutions of our national organism.'<sup>63</sup>

On this view of Conservatism, the Tory recognised the necessary inequalities of society. However, he also acknowledged a need to maintain organic relations between social classes; in contrast to the Liberal individualist, therefore, the Conservative sanctioned a form of paternalism which would remedy the most egregious causes of social discord. He also differed from classical Liberalism in asserting that it was only as part of the social whole that the individual could find fulfilment.<sup>64</sup>

This social philosophy had its cosmological counterpart in the new teleology. The latter found in nature the same reliance of the part upon the whole which formed the core of this brand of Conservatism. Further, the teleologists identified this structure with the will of God, thereby it became both good and necessary. It was the main feature of an over-arching design for man and the universe which was guaranteed by invincible power.

However, teleology was not the only route to this view of society. The negative aspect of the new teleology was a vigorous attack upon naturalism and, in particular, upon the notion that man and society were 'merely' natural. But it was not suggested that no inference from the natural to the human and social was possible; on the contrary, this was central to the positive side of the teleologists' case. When properly understood as the embodiment of God's purpose, nature was a highly serviceable resource in homilies about social order. In effect, what was denounced was a particular kind of naturalism: the naturalism of radical connotations. In its place was put a conservative naturalism. Moreover, such conservative naturalism could operate independently of the theological trappings with which Pringle-Pattison and others enveloped it.

Thus William Lilly insisted in 1889 that Conservatism rested on 'scientific' foundations. He held that the 'study of the arrangement of the natural organism may teach us valuable lessons as to the nature of the political organism'. In particular, such a study would reveal that 'Hierarchical science must of necessity be.'

Lilly went on:

Democracy will have to abandon its fond illusion of remaking the world in a day, or in a century, and to recognise as the law of the social organism, no less than of the individual, that binding together of old and new, the one handed down by heredity, the other added on by differentiation, which is of the very essence of evolution. So far the teachings of science are what may be called, in quite another than the partisan sense, Conservative.<sup>65</sup>

Here the comparison between the organism and society was put to a somewhat different use. Instead of unity and interdependence, the **inflexibility** of biological and social forms was stressed. Lilly's organicism exemplified a different notion of Conservatism than Parson's. To a degree these co-existed, together with their characteristic ideological forms, during the period between the home rule crisis and the First World War. However, as the social base of Conservatism changed during these years, so did its political orientation. At the same time, there was a basic and lasting alteration in the ideology of the movement.

### iii. The Constraints of Nature

[T]he cultivation of a high conception of what may be made of the world we live in, is capable of supplying a poetry, and (in a sense) a religion, much more fitted to exalt our feelings and ennoble our conduct than any philosophical, or mystic speculation regarding worlds unseen and unknown.<sup>66</sup>

(Montague Crackanthorpe)

In the words of one historian, the spread of socialism among the British working-class 'altered the whole character of the politico-economic debate.'<sup>67</sup> It also altered the alignment of political forces in Britain in the late nineteenth and early twentieth centuries. The long-standing contest for power between landowners and capitalists took place in the context of increasingly insistent attacks upon property and privilege of every kind. The agitation for land reform in Ireland and in Britain was seen as merely the first step to complete collectivisation. In the face of such a threat, William Graham remarked in 1895, all grades of landowner and capitalist could unite and the professional classes would follow their lead.<sup>68</sup>

The political outcome of this development has been discussed above: by the end of the century, the Liberal Party — which had supported Irish nationalism and which was increasingly receptive to social democracy — was deserted by many of its former adherents. These rallied to Conservatism: 'There they contributed to that amalgamation of land and capital, passive and active property, which was to dominate the social structure of politics in the succeeding age.' The Conservatives became identified as the upholders of all forms of property and as the most effective check upon the proposed degradations of the socialists.

This social and political shift had cultural consequences. Specifically, there were changes in the idiom and content of Conservative ideology. This was clear in W.H. Mallock's writings between the 1880s and 1890s; in the early part of the period,

Mallock's political writings ..... concentrated on defending the landed aristocracy's position and property rights against radicals by stressing its traditional paternalism and socialistic concern for the suffering of the masses. He characterised the radical business classes, on the other hand, as thoroughly selfish and motivated purely by the envy of the aristocracy. After

1890, however, he switched abruptly to supporting this very same capitalist middle class against socialists and his paternalistic attitudes gradually disappeared.

This transition was not just a result of Mallock's idiosyncracies: such defence of 'businessmen and their property typified official conservative thinking which Mallock, closely connected with the Conservative Central Office, both reflected and moulded.'<sup>69</sup>

In effect, the traditional Tory bias in favour of the landed interest gradually gave place to a policy orientated to the 'new Conservatives'. One consequence of this was the eclipse of the old-fashioned, aristocratic, organicism exemplified by Parsons; in its place appeared an ideology which took over many of the cultural resources of the middle-classes. An aspect of this shift was the changing use to which naturalistic arguments were put during these years. While in earlier decades scientific naturalism had served progressive political aims, in the 1880s and 1890s similar materials were adapted to another purpose: the sustenance of a Conservative attitude to change. The contrast can be characterised as one between an optimistic and a pessimistic naturalism. While the former world-view guaranteed or even demanded change; the latter imposed insuperable limits upon social improvement.

The transition was encapsulated in the changing political allegiances and utterances of such radical scientists of the 1860s and 1870s as Thomas Huxley and John Lubbock. These sided with Unionism in the 1880s; moreover, some of them reworked their scientific naturalism to make it useful to conservatism in the political controversies of the day.<sup>70</sup>

John Tyndall's commitment to radicalism had been less marked in the earlier period than either Huxley's or Clifford's. He had, for instance, disagreed with Huxley during the Eyre controversy, choosing to side with Eyre and the Conservatives against Huxley and the radicals. Similarly, Huxley and Tyndall had divergent views on the issues raised by the American Civil War.<sup>71</sup> Nonetheless, Tyndall was sufficiently sympathetic to the radical cause to have a high opinion of J.S.Mill as an MP.<sup>72</sup>

But by the 1880s whatever differences Huxley and Tyndall may have had on political issues were forgotten. Both came to oppose Gladstone's Irish policy and all that entailed. Huxley made his views public whenever possible and this drew the praise of his erstwhile adversary, St George Mivart. In 1886 Mivart told Huxley that at this time of crisis 'scientific men of mark' should 'come forward in

favour of rational, scientific politics'.<sup>73</sup>

Tyndall expressed a similar opinion. He wrote to Huxley in 1887 to suggest that the Royal Society (in which 'the majority of the Unionists is overwhelming') should take a stand in the home rule controversy. Tyndall proposed a joint declaration which read:

We, the undersigned attentive observers of passing events whose lives are, or have been, devoted to the cultivation of science, and whose tastes, interests, and aims are far removed from the temptations and turmoil of Party Politics, desire, at the present crisis, to record our deliberate conviction that the Irish policy of Mr Gladstone is fraught, not only with possible danger, but with certain disaster for the British Empire.

It is not only Mr Gladstone's general policy (calamitous as that would be, even if pursued on legitimate lines) that we deprecate. It is his encouragement of the demagogue; his virtual defence of tyranny which threatens the foundations of civilized society.<sup>74</sup>

Huxley concurred with the sentiment of the statement; he showed it to Joseph Hooker who was also in favour of it. After some discussion, however, the declaration was not published. Nonetheless, Tyndall continued to claim that there was a need for 'the free men and unbiased sons of science' to speak out on the issues of the day.<sup>75</sup> Huxley showed himself ready to take up this task; that is, to address contemporary questions 'scientifically'. He claimed to refer issues only to the facts of nature and denied that the conclusions at which he arrived contained any party or class bias.

As early as 1868 Huxley showed his awareness of the potential of naturalism as a resource in political and social discourse. In an address to the South London Working Men's Club he asserted the need for everyone to make some study of nature: 'that great university, the universe, of which we are all members'. Huxley compared this universe to a game of chess, where the 'chess-board is the world, the pieces are the phenomena of the universe, the rules of the game are what we call the Laws of Nature.' Everyone should learn these rules because nature punished those who transgressed against her rules; those who would not play the game would be 'plucked', and 'Nature's pluck means extermination.'<sup>76</sup>

A study of nature could therefore give rise to a set of normative injunctions which prescribed certain individual and collective actions and forbade others. For instance, a knowledge of the laws of nature would teach a man the

necessary connection between the moral law which prohibits stealing with the stability of society — by proving to him, once for all, that it is better for his own people, better for himself, better for future generations, that he should starve [rather] than steal.

Without some such natural basis for ethics, Huxley argued,

you have no foundation of knowledge, or habit of thought to work upon, what chance have you of persuading a hungry man that a capitalist is not a thief 'with a circumbendibus?' And if he honestly believes that, of what avail is it to quote the commandment against stealing when he proposes to make the capitalist disgorge?<sup>77</sup>

In 1880 Huxley restated this view of scientific knowledge as a means of social control to an audience of capitalists at the opening of Josiah Mason's Technical College in Birmingham. The 'definite order' of nature which science revealed was relevant not only to the management of things, through improved manufacturing processes, but also to the control of human beings. The prosperity of industry depended on

a clear understanding of the conditions of social life on the part of both the capitalist and the operative, and their agreement upon common principles of social action. They must learn that social phenomena are as much the expression of natural law as any others; that no social arrangement can be permanent unless they harmonise with the requirements of social statics and dynamics; and that, in the nature of things, there is an arbiter whose decisions execute themselves.<sup>78</sup>

In the course of the 1880s and early 1890s Huxley elaborated upon this argument. He developed the concept of nature as the arbiter which decided the limits upon social innovation as an answer to utopian socialists like Henry George and A.R. Wallace. The latter's work was especially important in defining Huxley's position.

Teleological cosmologies were not the exclusive property of Conservatives in late-Victorian Britain. Wallace argued from a natural world infused with spirit and direction to a socialist political philosophy. Wallace had in the 1880s abandoned the view that natural selection was a sufficient explanation of evolution of man. The advent of *mind* in man 'changed the whole course of evolution.

The latter conclusion implied that when man acquired his mental attributes, his body was no longer susceptible to natural selection. Instead, man adapted by intellectual control of his environment and by his development of benevolent social relations. The mental means of control and the social relations therefore became liable to selection.<sup>79</sup> Through his possession of a rational mind man became, in part, **super**-natural: above the processes of Nature which had given rise to his body. Further, he now shared in the intellectual quality of the author of those processes, whose purpose — 'the only **raison d'être** of the world ..... was the development of the human spirit in association with the human body.'<sup>80</sup> If a higher intelligence had guided the laws of Nature in order to create other intelligent and moral beings, then a consistency between human needs and aspirations and the means of their satisfaction might be anticipated: God having provided for his creatures. Thus Wallace argued that through his greater understanding of Nature man would acquire knowledge both of what God intended for him; and the means of attaining those goals, which he identified with a more free and equal society. Given moral mind's sovereignty over Nature, the only impediment to the achievement of a just society was the reluctance of certain persons to part with the wealth that needed to be redistributed in the construction of such a social system.<sup>81</sup>

Wallace used these cosmological arguments to support his advocacy of schemes for land reform, and for the redistribution of wealth more generally. Huxley's hostility to socialism led him to reject this programme of reform, and to seek an answer to Wallace's appeal to teleology and the primacy of mind in Nature. At the same time, his aversion to absolutist political theory, and his admission of the need for some measure of state interference with society prevented Huxley from adopting the simple expedient of confronting Wallace's socialistic teleology with an individualist one, such as Spencer's. Instead he developed a distinction between ethical obligation and natural necessity which was intended to restore the diremption between what men ought to do and what they could do.

In 1888, Huxley published 'The Struggle for Existence in Human Society'<sup>82</sup> in which he advanced a concept of Nature which invalidated socialism as a practical political programme. Firstly he considered the argument that the evident harmony of Nature is evidence of its contrivance by a beneficent being; and concluded that such an 'optimistic' interpretation of Nature was superficial and misleading.



In sober truth, to those who have made a study of the phenomena of life as they are exhibited by the higher forms of the animal world, the optimistic dogma, that this is the best of all possible worlds, will seem little better than a libel upon possibility .....

[E]ven the modified optimism of the time-honoured thesis of physico-theology, that the sentient world is, on the whole, regulated by principles of benevolence, does but ill stand the test of impartial confrontation with the facts of the case.<sup>83</sup>

If one contemplated the random allocation of pleasure and pain among living things, one was led to conclude that the course of Nature appears 'neither moral nor immoral, but non-moral.'<sup>84</sup>

Moreover, this amoral aspect of Nature extended to all living things, in as far as they were the products of evolution. They developed their specific characteristics in response to the contingencies of their environment; however, there was no necessary correlation between such change through adaptation and progress. For evolution 'depends on the nature of those [environmental] conditions whether the direction of the modifications effected shall be upward or downward. Retrogressive is as practicable as progressive metamorphosis.'<sup>85</sup> The direction of organic development as described by biology was therefore as little imbued with any ethical import as the trajectory of a projectile as described by the laws of mechanics. Neither corresponded to any simple pattern of uniform motion; both involved a rise and a decline: 'and the sinking half of that course is as much a part of the general process of evolution as the rising.'<sup>86</sup>

Having so dismissed the 'pleasant dream' of the school of Leibniz; Huxley went on to deny that the hideous 'nightmare pessimism of Schopenhauer is any more consonant with the facts of sentient existence [than] ..... optimism.'<sup>87</sup> At this point in his argument Huxley deviated from his earlier position in an attempt to present a description of reality which, he claimed, avoided both these excesses. Strictly speaking, he said, 'Nature' denoted 'the sum of the phenomenal .....; and society like art, is therefore a part of nature'. But Huxley then drew an, apparently, most significant distinction between 'those parts of nature in which man plays the part of immediate cause', and the rest. By this criterion, Huxley decided that 'society, like art, is usefully to be considered as distinct from nature.'<sup>88</sup> What made this distinction 'useful' was that it enabled Huxley to designate a sharp divide between the realms of the moral and of the non-moral and to draw important conclusions from that distinction.

The social man was essentially different from the natural man because he had a 'definite moral object', and this entailed wholly novel modes of behaviour to those adopted in nature. The transition from mere animal to man involved the overcoming of natural patterns of behaviour and the substitution for them of ethically directed action. The former state was one in which self-assertion against others for the possession of the scarce means of existence was the overriding imperative. But in society, man's condition *qua* ethical being was a 'negation of the unlimited struggle for existence. He [man] tries to escape from his place in the animal kingdom, founded on the principle of non-moral evolution, and to establish a kingdom of Man, governed upon the principle of moral evolution.'<sup>89</sup>

By so agreeing that there was a dysjunction between human society and nature, Huxley apparently conceded Wallace's central contention — that the fact of constant struggle among animals did not entail similar conduct between men. But this was a concession that was immediately qualified in a crucial manner. Because, while man in society *aspired* to be moral, these efforts at civilisation 'by no means abolished, perhaps [have] hardly modified, the deep-seated organic impulses which impel the natural man to follow his non-moral course.'<sup>90</sup> Here the full significance of Huxley's initial denial of teleology in Nature became clear. Since Nature embodied no ethical purpose, human ideals were alien to it; if they were to be realised at all, these had to be realised by human effort alone — exerted in most cases in opposition to the natural. But because man was himself part of the natural, he shared its non-moral character in certain fundamental aspects of his being. In consequence humanity was divided between an ethical element, which was unique to it, and impulses which were held in common with the rest of the animal world. While the former might tend towards a moralisation of his conduct; the latter opposed any such trend. The realisation of the ethical in society could never be more than partial as a result of this bestial residue in human nature. Specifically, the natural condition of competition and conflict could never be entirely replaced by the ethical state of cooperation and peace.

Huxley identified the most fundamental of these causes of regression: namely, 'the tendency to multiply without limit, which man shares with all living things ..... [I]n civilized society, the inevitable result [of this tendency] ..... is the re-establishment in all its intensity, of that struggle of each against all the mitigation or abolition of which was the chief end of social organization ..... Nature appears to have ..... little sympathy with the ends of society.'<sup>91</sup> Despite her amorality and lack of purpose, Nature

remained potent in human affairs; in fact the ultimate arbiter of how far men could progress in realising their ideal ends. Man had risen far enough above other animals to conceive of a higher state than the natural; but his retention of a propensity to unlimited reproduction ensured that his strivings toward an ethical society would necessarily be thwarted. In the 'decay of states and the foundering of civilizations ..... no doubt immoral motives of all sorts have figured largely among the minor causes of these events. But beneath all this superficial turmoil lay the deep-seated impulse given by unlimited multiplication.'<sup>92</sup>

Because of this tendency for the natural to overwhelm the moral instincts in men, the hopes of those 'finer spirits' who looked forward to a city of God on earth, when competition and a struggle to survive would be replaced by cooperation and the satisfaction of everyone's needs, were doomed to failure. So long as the 'natural man' continued to reproduce more quickly than he increased the means of subsistence; 'so long will peace and industry not only permit, but they will necessitate, a struggle for existence as sharp as any that ever went on under the 'regime' of war.'<sup>93</sup> This fact pressed an unavoidable imperative upon every polity: to feed its growing population as its first priority. Huxley spelt out the implications of this for his own society, whose home produce was sufficient to feed only half of its inhabitants; to survive, Britain had to be competitive industrially. It had to produce the goods which food producers wanted at prices that they were prepared to pay: 'We not only are, but under penalty of starvation, we are bound to be, a nation of shopkeepers'.<sup>94</sup> The question of the distribution of wealth which social justice might require had to take second place to the question of how production must be organised to satisfy the demands of Nature, acting in this instance through the agency of the world market. The form of social organisation that most fully met these demands must be maintained, and whatever injustices that it entailed be endured.

Let us be under no illusions, then. So long as unlimited multiplication goes on, no social organization which has ever been devised, or is likely to be devised, no fiddle-faddling with the distribution of wealth, will deliver society from the tendency to be destroyed by the reproduction within itself of that struggle for existence the limitation of which is the object of society. And however shocking to the moral sense this eternal competition of man against man and nation against nation may be; however revolting may be the accumulation of misery

at the negative pole of society, in contrast with that of monstrous wealth at the positive pole; this state of things must abide, and grow continually worse, so long as ..... [Nature] holds her sway unchecked.<sup>95</sup>

In this way inequality of wealth and the existence of poverty were presented not as features of society which were contingent merely upon the interests of those groups which benefited most from them; but as essential conditions of social survival, sustained not by men but by the world within which men must live.

Huxley specified those aspects of social organisation which were thus founded upon natural necessity in more detail. Among them were low wages (to keep British goods competitive), and the consequent measure of misery that such social reformers as Wallace and George had thought to abolish. Nature had decreed for a proportion of the population 'a life of unsuccessful battling with hunger and a pauper's grave'. Huxley admitted that if wages fell too low, this impoverished portion of the work-force might grow so large as to threaten to disrupt society. But the real danger to the social order arose when people failed to see that such suffering was natural; but came to regard it as the result of remediable conditions. Then, 'men naturally enough begin to think it high time to try a new experiment.' Such speculations were perhaps inevitable in a state of society where poverty was as widespread as it was in late-Victorian England. But by placing this issue within its cosmological context, Huxley believed that he had established the limits within which practical proposals to alleviate this problem must proceed; and that schemes which went beyond these limits were bound to fail, and to fail disastrously.

The reforms which Nature would permit were mere palliatives: better drainage of poor areas; bath-houses for the inhabitants; gymnasia and libraries to keep them amused. Such steps Huxley was ready to countenance because they were 'not only desirable from a philanthropic point of view, but an essential condition of safe industrial development'.<sup>96</sup>

A similar intent to discredit socialistic solutions to the problem of poverty and to vindicate a slightly qualified version of 'laissez-faire' capitalism is found in the series of articles Huxley published in 1890. 'Capital — the mother of Labour: an Economical problem discussed from a physiological point of view' was an attempt to refute George's labour theory of value and to dissolve the political delusion that

'labour and capital are necessarily antagonistic.'<sup>97</sup> In the course of the essay Huxley tried to assimilate economic to physiological processes; and to show that in both, 'the life of man is dependent upon the pre-existence of a stock of material, which is disposed in such a manner as to be utilisable with facility. And I further think that the propriety of the application of the term 'capital' to this stock of useful substance cannot justly be called into question .....'<sup>98</sup> In further controversion of George's claims Huxley insisted that the value of the articles that labour produced from this stock depended, not upon the work done, but upon their market worth. It was therefore left to the capitalist to decide whether it was sufficiently profitable for him to hire men to work upon his stocks, after considering what demand there was likely to be for their products. Huxley continued:

I fail to discover any *a priori* "rights of labour" in virtue of which these men may insist on being employed, if they are not wanted ..... I think it is clear that there is only one condition upon which the persons to whom the offer of these "wages" is made can accept; and that is that the things offered in exchange for a year's work shall contain at least as much vital capital as a man uses up in doing a year's work ..... there is an irreducible minimum of wages; it is such as suffices to replace the inevitable consumption of the person hired.<sup>99</sup>

But even this bare minimum which was all that social justice demanded that a worker should receive for his labour, could not be guaranteed. Again the 'Malthusian doctrine' had to be recalled; and it was necessary to realise that under certain conditions, when unchecked reproduction had had its effect, 'a time must come when some will have to starve'. This natural basis of poverty was Huxley once more asserted: 'the fundamental condition of the existence of any polity, or organised society of men'. In the face of Nature the 'rights of labour', such as they were, had only limited scope for realisation.<sup>100</sup>

In two further articles, 'On the Natural Inequality of Men' and 'Natural Rights and Political Rights',<sup>101</sup> Huxley developed his attack on socialism by placing George's theories in the *a priorist* tradition of Rousseau, and by attacking this form of political discourse through his naturalism. The fault of such rationalist social theory as Huxley saw it was that it claimed that 'human suffering must needs be the consequence of the artificial arrangements of society and can all be alleviated or removed by political changes.'<sup>102</sup> This 'fallacious guidance' was now being offered to 'English workmen of the better class'; Huxley sought to counter its influence before 'mistaken theories' were translated into 'illegal actions'.<sup>103</sup>

The error of Rousseau and of those who followed him was to draw a distinction between 'natural' or physical inequality which is 'established by nature', and 'moral' or 'political' inequality that depends 'upon a sort of convention'. Huxley objected to this contrast that it did not ask whether the two forms of inequality were not, in fact, closely connected, 'in such a manner that the latter is necessarily a consequence of the former'.<sup>104</sup>

It was Huxley's contention that such a connection did indeed exist, and that all attempts to enunciate absolute principles which purport to be 'at once ethically and politically sufficient rules of conduct' should be subject to the check of a reference to reality: to the facts of Nature.<sup>105</sup> Rousseau, Huxley claimed had 'sagaciously fought shy ..... of the question of the influence of nature upon political equality';<sup>106</sup> and only in consequence of this wilful neglect had he been able to reach the conclusion that all men were born free and equal. In truth, the ordering of society, 'with its rights of property, and its practical distinctions of rank and power ..... all [come] ..... about neither by force nor by fraud, but as the necessary consequence of the innate inequalities of capability'.<sup>107</sup> Those who were worse endowed by Nature must serve the more fortunate. 'So long as men are men and society is society, human equality will be a dream; and the assumption that it does exist is as untrue in fact as it sets the mark of impracticability on every theory of what ought to be, which starts from it'.<sup>108</sup>

This was the crux of Huxley's objections to a *priorist* social theory. Its proponents 'busied with deduction from their ideal "ought to be" ..... overlooked the "what has been", "the what is", and the "what can be" '. Facts must take precedence over, and perhaps determine, values if social theory was to be an influence for good instead of a malignant irritant of men's discontents.<sup>109</sup> In 'Natural Rights and Political Rights' Huxley made clear that his concern to refute such fallacies had immediate and practical roots. For 'the vicious method of a *priori* political speculation ..... is not only in full vigour, but ..... is exerting an influence upon the political action of our contemporaries which is extremely serious'; especially grave was its influence upon the 'more intelligent of the working classes'. In common with Rousseau, George proceeded from dubious axioms to 'upset the existing arrangements of society'.<sup>110</sup>

If an historical school of political thought should be taken for a model, then it should be the physiocrats, not the 'a priorists'. To the physiocrats was owed 'the modern clearness of conviction that the world of human society is as much the theatre of order and definite sequence of cause and

effect as the world of extra-human nature; that there are rules of action, the obedience of which brings about prosperity, while their neglect entails ruin, which have nothing to do with the laws of morality or the ordinances of religion; and that the wicked who follow these rules will not beg their bread, while the pious who neglect them will.<sup>111</sup> It was such prudential maxims that Huxley's discussions of politics, economics and society were intended to produce. The general force of such precepts was derived from his contention that moral terms were inapplicable to Nature: 'in nature, fact and justification of the fact, or, in other words, might and right, are coextensive.'<sup>112</sup> It was pointless therefore to speculate on what ought to be the case in a moral world; but necessary to take note of what is, and to act accordingly. The particular force of these maxims — that is, their relevance to the issues which Huxley wished to influence — lay in his claim that modern man in his most basic activities of production and reproduction of the species remained a slave to this amoral Nature. Unchecked the operation of this law of Nature tended to purely selfish actions; it led to war and competition between men — 'to the sum of all possible anti-social and anarchic tendencies'.<sup>113</sup> Humanity might struggle against these tendencies through social organisation and the cooperative morality that it entailed; but the struggle must ultimately fail since man remained fundamentally a 'Natural' being.

That natural selfishness that humanity had inherited from its primate ancestors was 'the reality at the bottom of the doctrine of original sin'<sup>114</sup>; it was the reason why although he may attain a 'civis non diabili', the 'civis Dei' must ever lie beyond man's reach though not beyond his conception. This was why those who believed with Rousseau that 'the golden age would return if only the State directed production and regulated consumption; and that the love of approbation affords a stimulus to industry, sufficient to replace all those furnished by the love of power, of wealth and of sensual gratification, in our present imperfect state', were wrong. It was because Huxley thus both acknowledged a society based on cooperation as the goal of social morality and denied that this goal could be realised, that his later writings came to be labelled 'pessimistic'.

But this pessimism had a polemical intent. It was designed to curb the claims of 'optimists' about the potentialities for social amelioration. Clear thinking on social matters, Huxley declared, required the initial abolition of such 'philosophical delusions' as he had tried to refute. From this destructive exercise he hoped a 'sane social philosophy' might proceed.<sup>115</sup> The basis of such a philosophy would have to be

the realisation that 'the natural order of things ..... does not tend to bring about what we understand as welfare. On the contrary, the natural order tends to the maintenance, in one shape or another, of the war of each against all'.<sup>116</sup>

Huxley's concern to establish such limits was derived from his concern that untrammelled optimism would contribute to the social unrest of his time. The political philosopher 'who uses his 'a priori' lever, knowing that it may stir up social discord, without the most conclusive justification, ..... comes perilously near the boundary which divides blunders from crimes.'<sup>117</sup> Huxley was fond of quoting Robespierre and St. Just as examples of what might occur if such principles ever became the bases of political action. His instrument in trying to counter such an evil influence was the concept of nature that has been described. His location of society within that concept enabled Huxley to define the issues in question in such a way as to provide determinate answers to the social questions of his day. 'The political problem of problems', according to Huxley, was 'how to deal with over-population, and it faces us on all sides.'<sup>118</sup> The causes of poverty, thus explained, were natural and beyond social remedy.

The main function of 'nature' in Huxley's political and social thought was therefore that of a negative constraint upon action. To this end, he denied teleology of Wallace's kind, but stressed the power that the natural and non-moral aspects of man had over society. The human condition was one of pathetic and futile struggle against chains which had been forged during aeons of evolutionary history. Those 'political speculators', who failed to acknowledge the limitations upon man's achievements that his provenance imposed, but who promised 'a millennium of equality and fraternity', had reckoned sadly without 'their host, or rather hostess, Dame Nature.'<sup>119</sup>

This kind of conservative naturalism was widely used in Britain at the turn of the century. Its various manifestations served a single interest: the elevation of certain social contingencies to the status of natural necessities. As Mallock wrote in 1883, it was not enough to refute socialist doctrines on the grounds that

the upper and middle classes are shocked by them; but because they are founded upon some scientific falsehoods — the perversion of some broad natural fact: and they must be capable as such of being stated in accurate terms, and confronted with the truth, equally



distinct, that corresponds to them.<sup>120</sup>

Two years before Mallock had complained at the lack of a 'science of human nature', a body of 'objective' knowledge which would still the current social ferment by answering the pressing question of the day. Namely, 'how far are certain things removable, which a certain set of men are clamouring to have removed? How far, for instance, can we remove social inequality?' <sup>121</sup>

One means of defending inequality has already been noted. In organicism the individual existed only as part of a whole; because each of these parts was suited to a particular function there were necessarily differences between them. However, there was another type of Conservative rhetoric in late Victorian Britain which defended inequality from a quite different model of society. Thus, **contra** holism, Mallock insisted that society was the sum of the individuals who composed it; it was therefore dependent upon the character of its human constituents. Those features of society which were constant were the expression of 'the most permanent features in human character.' Of these 'the most permanent has been inequality.' In consequence, society could only change if human beings changed; there were, however, limits to the elasticity of human nature.<sup>122</sup>

Mallock later went further and argued that social structure could be ascribed directly to the natural differences between individuals.

These differences divide them not into isolated individuals, but into a large number of classes. These classes are distinguished from one another by the degree and kind of talents with which their members are endowed.

In fact, each class had its 'typical' individual who embodied the traits which fitted it to a particular social status.<sup>123</sup>

A defence of hierarchy was thus combined with a restrictive view of the prospects for social change. Such generalised arguments were often accompanied by more strictly 'scientific' demonstrations of the same point. For example, in 1891 A.J. Balfour set out to challenge all utopian theorists who had postulated the possibility of unlimited improvement for all mankind. One source of such hopes had come from 'optimistic' evolutionary theories like Herbert Spencer's. These suggested that the modifications in mental and physical characteristics acquired by one generation could be transmitted to its posterity and that 'by the cumulative effect of such changes, profound alterations may be made in the character

of the species.' Balfour tried to counteract such optimism by a pessimistic naturalism which stressed the **stability** of the human specific type. The doctrine of the inheritance of acquired traits had, he claimed, been discredited by August Weismann's demonstration of the distinction between the 'germ plasm', which transmitted characters from one generation to the next, and those parts of the body which interacted with the environment. No plausible mechanism had been suggested whereby somatic alterations might have genetic results.<sup>124</sup>

There were therefore no grounds for supposing that 'natural man' might undergo a continuous process of improvement and the existing checks upon social innovation be so superseded. On the contrary, the evidence pointed rather to the immutability of species over vast periods of time. Balfour concluded that the physiological makeup of man was effectively fixed and that such improvement as might occur was 'strictly conditioned by the quality of the stuff to be worked on.' The dreams of a millennium when 'there shall not only be a new heaven and new earth, but also a new variety of the human race to enjoy them' were futile.<sup>125</sup>

On this and on similar occasions Balfour readily conceded the amorality of nature; a fact which, in other contexts, he found so objectionable. In effect, Balfour's work contained two **prima facie** incompatible elements which made up so much of the Conservative rhetoric of the day. On the one hand, there was the teleology and organicism of the old Tories; on the other, the naturalism and atomism of the new.

Both paths tended to lead to a vision of society as hierarchical and unamenable to change. However, it would be wrong to suppose that these two idioms simply co-existed in the polemic of the period. The 'new'-style Conservatism gradually displaced the old; this cultural change reflected the altered character of the Conservative Party and the interests it represented.

In his plea for organicist Toryism, Harold Parsons had recognised that his emphases were rejected by certain parts of the Conservative Party. The new, middle-class Conservatives who had been driven to the Party by 'fear of radical spoliation' were, Parsons alleged, exerting a baleful influence on the movement.<sup>126</sup> L.T. Hobhouse in 1904 similarly noted the 'great transfer of material interests' to the Tory Party which had occurred in the past decade. He also described its political consequences. The 'suburbanites' who now filled the Conservative ranks cared nothing for social reform: 'All they know of

social and domestic reform is that it means expense, and their politics is summed up in the simple and comprehensive formula — keep down the rates.<sup>127</sup>

This was the assessment of a hostile observer. However, Conservative writers agreed that in the first decade of the twentieth century the defence of property was the prime goal of their Party, and that this had become largely a matter of the level and character of taxation.<sup>128</sup> The demand of middle-class voters that government exactions should not be increased imposed strict limits upon 'Tory Democracy'. While the rhetoric of the social organism, in which the aristocracy tended to the needs of the other classes, retained a certain value; it bore little relation to the practical orientation of Tory policy. At the 1895 general election Balfour pledged the Conservative Party to 'Social legislation' to remove the worst working-class grievances. He argued that socialism would 'never get possession of the great body of public opinion ..... amongst the working classes or any other class, if those who wield the collective force of the community show themselves desirous .... to ameliorate every legitimate grievance and to put society upon a more solid basis.' This was the language of Tory paternalism. However, when elected, the Unionist government of 1895 - 1900 did little to put these promises into effect, even though the need for welfare measures became greater as unemployment rose. Paternalism, if taken seriously, would cost money, and Conservative leaders took heed of their supporters' opposition to such extravagance.<sup>129</sup>

But the Liberal Party was prepared to buy off the working-class with expensive concessions. Rightly, the Liberals feared that if they did not show themselves to be a party of social reform, that role would be assumed by the new Independent Labour Party and other socialist bodies. In addition to this parliamentary threat was the fact that the working-classes showed themselves to be more and more ready to use direct action to gain their ends. Social security spending seemed to Liberals like Lloyd George and Churchill in the long run the cheapest and safest course.<sup>130</sup>

Conservatives, in contrast, looked for measures which would frustrate socialism at a lower price. Balfour indicated one major direction in which this search would proceed in a speech to the House of Commons in 1906. He criticised politicians who rushed into schemes for the relief of urban poverty before they had fully understood the issues. To be effective, policy must rest on 'some really solid scientific basis' from which the true nature of deprivation in the cities could be judged. In particular,

politicians needed to acquaint themselves with the facts concerning the relative influence of heredity and environment on successive generations.<sup>131</sup>

One way to combat the reforms advocated by Liberals and socialists was to build up contemporary theories of heredity into the foundations of a social policy. If Weismann was correct, environment played little part in determining the permanent features of organisms; the true causes of an individual's constitution were the immutable traits that he had inherited from his parents. There was a movement in early twentieth century Britain to number the propensity to chronic illness, to unemployment and to poverty among these hereditary characters. On this view the real roots of deprivation lay not in the structure of society but in the sum of such traits present in the population. The remedy rested not with environmental reform but in a policy of eugenics to reduce the proportion of such undesirables in society.<sup>132</sup>

In 1907, the year after a Liberal government pledged to 'social democracy' came to office, the Eugenics Education Society was founded to press this alternative approach to the social problem. As one of its founders wrote, the establishment of the beginnings of a welfare state by the Liberals launched Britain

on a sea of uncertainty which occasions great uneasiness to many persons and especially to those who have families to support. What with free education at one end and the prospect of greatly increased burdens at the other, John Bull is being sorely tried. Whilst he justly seeks, by a fervent appeal to patriotism, to encourage the reasonable multiplication of the fit, with equal justice he demands that some control should be exercised over the unreasonable multiplication of the unfit, whether such unfitness be due to drink, feeble-mindedness, insanity, criminality, or disease.<sup>133</sup>

In these words Crackanthorpe summarised both the goals and the means of the eugenic movement. The most vocal elements among the members of the EES were inclined to Conservatism and were loud in their condemnation of Lloyd George's measures and of socialism in general.<sup>134</sup> On the one hand, the EES called for a reduction of the fertility of the 'residuum', the genetically unfit members of society who were a burden on the rest. On the other, the eugenists demanded that the fit, and especially the middle-class, be encouraged to have more children through a reduction of the tax burden on them.

These proposals embodied the resistance of Conservatives to the demands of an importunate proletariat and the opposition to any attempt to relieve the condition of that class by a redistribution of wealth.

As such, eugenics stood as the culmination of the conservative tendency of scientific naturalism at the turn of the century. It embodied in a highly developed form the notion of man as a symmetrical aspect of nature and of society as the product of his natural character. Eugenics was naturalism made practical: cosmology was intertwined with a set of policy orientations which were held to follow from a scientific understanding of humanity.

In the case of the 'official' line, as embodied in the statements of the EES, this orientation conformed closely with that of the Conservative interests in society whose aim was to inhibit the development of a welfare state. This type of eugenics was an aspect of the Conservative polemic of the first decade of the twentieth century designed to discredit socialist schemes in the country.<sup>135</sup> However, it should be noted that there were other versions of the eugenic creed current at the time. Balfour, who supported the EES and became its honorary vice-president in 1913, saw that, in addition to its Conservative uses, eugenics had become part of the rhetoric of 'radical' elements in society. He dissociated himself and the EES from 'faddists' who seized upon eugenics as a means of 'bringing about the millenium upon earth.'<sup>136</sup>

These faddists included some who called themselves 'socialists'. Karl Pearson, the chief ideologue of eugenics of the period, had at one time regarded his philosophy as socialistic. However, his use of the term explicitly excluded Marxism, syndicalism and other revolutionary creeds. Instead, Pearson regarded socialism as a form of statism; as the rationale of a more active involvement of government in society. Eugenics, with its stress on the need to manipulate differential fertility, legitimated one form of intervention.<sup>137</sup>

Pearson was in most respects exceptional. Nonetheless, other socialist thinkers in Edwardian Britain shared at least some of his views. Thus Fabians like Sidney Webb also held socialism to be a creed of state involvement in society. Moreover, Webb regarded eugenics as supplying a prime example of the form this intervention might take: he wrote in the *Eugenics Review* of 1910 of the need 'deliberately to manipulate the enviroment to improve the stock'. Eugenics was thus turned to

'progressive' as well as to 'reactionary' political goals in Edwardian Britain.<sup>138</sup>

Such eugenists as C.W. Saleeby hoped to rescue eugenics from 'its present state as a class movement and a cover for selfish opposition to social reform.' However, the advocacy of social reform by progressive eugenists was no less a vindication of the interests of a certain section of society than conservative eugenists' opposition to welfare spending. While the latter suited the bulk of Conservative voters whose main aim was to stabilise or reduce taxation, the former conduced to the interests of the professional elites who would be called upon to implement social democratic policies and to manage the welfare state.<sup>139</sup>

This divergence indicates an important differentiation which was taking place in the anti-proletarian alliance in early twentieth century Britain. After the initial shocks of the 1880s and 1890s which had brought about considerable unanimity between disparate and previously competitive groups, more special interests began to assert themselves in the decade prior to the First World War. Specifically, two alternative responses to working-class demands were developed. The first, as embodied in official Conservative policy consisted of simple opposition to all attempts to secure even a modest redistribution of wealth; these, Conservative leaders feared would be the first step to revolution. As Balfour gloomily remarked in 1906: 'what is going on here is a faint echo of the same movement which has produced massacres in St Petersburg, riots in Vienna, and Socialist processions in Berlin.'<sup>140</sup> Lloyd George and his like were by their concessions to the proletariat opening the way for similar upheavals in Britain.

The other response was in the long run more significant. It recognised that contemporary conditions demanded a more flexible form of social control. On this view, a 'collectivist' social policy, which engaged in extensive relief schemes to defuse working-class unrest, was the most effective way of preserving the essential structure of society. While the first attitude was closely connected with the attitudes of the Conservative Party, the second was associated first with the 'New Liberalism' and then with the social democratic or Fabian wing of the Labour Party.

Besides their general political function collectivist policies served the interests of particular social groups: in Peel's words,

an important element in the rise of collectivism was the growth, owing to the enormous

expansion of technical and administrative tasks, of the professional, as against the entrepreneurial middle class. In Chadwick's day professionals were the main agents of the silent expansion of government, and eventually became, in the Fabian Society, the prime advocates.<sup>141</sup>

The subsequent development of this form of social control cannot be considered here. In conclusion, however, it should be noted that this collectivism made use of a sublimated form of naturalism; it assumed that there was a close analogy between the social and the natural worlds and that the methods applicable to the one had validity in the other. Just as nature had been made more amenable to human needs by the application of specialist knowledge to particular areas of concern, so society could be improved by similar techniques. In its early manifestations, in Pearson's work for instance, this ideology was underpinned by an elaborate cosmology in which the continuities between the natural and the social worlds were made explicit. In the twentieth century other 'progressive' eugenicists advocated their policies by the claim that they promised the 'rational control of man over the natural laws of evolution to which he has hitherto been subjected.'<sup>142</sup>

With time, however, the overt cosmological justification for technocracy became otiose. Its presumptions had become institutionalised; they were embodied in the practice of government and idealised in the mainstream of western sociology. Naturalism had ceased to be a social programme and had become a social reality.

### Concluding Remarks

The preceding chapters have presented scientific naturalism as a strategy of the new professionals in Victorian Britain. In particular, naturalism has been described as part of the efforts to establish science as a professional institution during this period. This interpretation has been supplemented by taking cognisance of a broader range of events which are also relevant to an understanding of the social meaning of naturalism. The use of naturalistic cosmologies to other professional groups has been outlined, as well as their wider political significance.

Each chapter, individually, has stressed one or more aspects of these naturalist strategies. Together, they are intended to provide a composite representation of something of the complex dynamic of nineteenth century society. In general that representation reveals the appearance of new specialist groups within the middle-class who sought to fortify their corporate identity, and who entered into competition with established elites for the means of privilege and power; among the instruments of this consolidation and competition were the resources of scientific naturalism. Within this framework the affinity between such evidently disparate activities as abstruse debates over the organism and public controversies over the relation of mind and matter becomes apparent. All were aspects of the attempts of a portion of the new professionals to achieve internal definition, external integrity, and access to enhanced resources.

For most of this narrative, those sections of British society which lay outside the ruling-classes have received little attention. The 1860s and 1870s were a period of relative social calm in Britain. There was a lull between the crises of the years from 1815 - 1849 and the great fear of the 1880s brought about by economic growth and by the admission of at least part of the working-class into the constitution. Because of this relaxation of the basic tensions of British society, it was possible for the local competition between professional groups and the skirmishing between the landed interest and its enemies to proceed without major distractions.

The final chapter has discussed how this changed in the last decades of the nineteenth century. It does not attempt to say why this change occurred: the social and economic shifts which underlay the political tensions of the period have been too little studied to permit such an explanation. What has been shown is a growing concern with working-class unrest in the 1880s and 1890s and the cultural



consequence that flowed from it. I have placed greater emphasis upon the impact of proletarian disturbances, the rise of socialism, and the increased militancy of the trades unions than is conventional; most historians have tended to imply that these developments posed a relatively minor threat to the social order, and that the real challenge came only in the decade prior to the First World War. The justification for my departure from this view lies in the prevalence in the writings of contemporaries of a quite different perception of events. Those living in the 1880s and 1890s did not compare current events with the 'objectively' greater dangers of 1911; instead they contrasted them with the supposed peace between the classes in the previous twenty years. The 1860s and 1870s were idealised in order to heighten the contrast — such anomalies as the riots of 1866 - 67 were ignored.

The 1880s were regarded as a period in which this peace was shattered by inflated working-class demands, encouraged by radical demagogues, which if unchecked would lead to revolution. Under these conditions the quarrels of the preceding decades did not disappear; they did, however, become less important. More energy was devoted to fending off the greater menace. In the process, the social meaning of naturalism changed in the way that has been described.

There is no 'conclusion' to this thesis. The general points I wished to make have been rehearsed sufficiently and there is no point trying to summarise the detailed argument. The thing must come to an end though, and perhaps the most appropriate terminus is a statement of what has been left unsaid or which needs further elaboration.

I have aimed at an overall view: at a recognition of the interconnection between a variety of cultural products within a cohesive social movement. As a result it has been impossible to stop to give certain topics the individual attention they deserve. To have done so would have required abandoning this project and undertaking another. As it is, I hope that the general interpretation offered here will suggest orientations for more specific studies. These may, in turn, cause a revision of some of the judgments. Nonetheless, I believe that the model of explanation used in this account does supply a strategy of historical investigation which will lead to a fuller understanding of the place of science in Victorian culture.

The most exciting possibility which arises is for the systematic study of the development of 'research schools' in the nineteenth century. Some version of this concept is familiar to historians of

science: Gerald Geison's study of Michael Foster and Cambridge physiology is a leading example of how much it can be made to yield. However, the full potential offered by an investigation into the development of schools has not yet been realised. A well-established school is a complete institution; a fraction of society which possesses certain resources. The way in which it acquired these resources affords a framework within which the main issues of the relation of science to its wider social context can be addressed.

To exist, a school must negotiate a space for itself in society; it must find a place among other institutions and this entails action on several levels. There is a need to obtain the material means to pursue a certain kind of science. This involves an adjustment of interests with other specialities; but it also casts the scientist into the thick of the wider political melee of his society. To survive and prosper in this contest a science needs influence and power; to gain these it cannot avoid aligning itself among the factions of the time.

Just what this alignment involves will vary between contexts. However, it is important to remedy the bias that arises from a late-twentieth century perspective on such proceedings. In a society where science is a fully-integrated part of the governing technocracy, with guaranteed resources, there is a tendency to underrate the influence of the political on the scientific. Where science is struggling for recognition, this relationship is far more visible. Even today the autonomy of science is more apparent than real; it cannot escape the influence of the wider nexus of interests which have originated and maintained the scientific enterprise.

The other major level at which the creation and perpetuation of science proceeds is the distinction and maintenance of the symbolic materials which constitute the subject-matter of a research school's activity. The creation of this subject-matter is, in all cases, an active affair: it involves a choice between various options to make nature one thing rather than another. This choice is not made randomly; it can be referred to the goals of the actors involved. Which goals are relevant to such choices, and from which social context they arise, are contingent matters. But, in principle, no social interest can be excluded as *a priori* irrelevant.

This raises the crucial question of how the two 'levels' of scientific institutionalisation are related. What, if any, is the connection between the acquirement of material and symbolic resources? In

particular, to what extent must the content of scientific knowledge be explained in terms of the political environment in which it is generated?

This thesis has shown how in nineteenth century Britain nature was a means of articulating interests in the maintenance or erosion of hierarchies. In this sense the natural world was certainly endowed with political meaning. Such strategies should be regarded as late instances of a form of social discourse that can be traced back to the sixteenth century in England in which similar cosmological categories embodied conflicting claims about the distribution of power in society.

However, the importance of the nineteenth century lies less in these continuities with the past, which have been noted in the preceding pages, than in the new social organisation of the study of nature during this period. The nineteenth century saw the beginnings of that intense specialisation which distinguishes modern industrial society. 'Science' was built up during these years as a particularly esoteric specialism. The creation of the cultural boundaries that professional science demanded was, to a large extent, what naturalism was about.

The greater social distance between the scientist and the rest of society was matched by an epistemological distance. Science lost its earlier intimacy with what were now distinguished as 'philosophy' and 'theology'. It has been widely assumed that, in the process, it lost its wider societal significance. While philosophy and theology might well have continued to express social interests, science was untouched by such concerns.

It is important to separate what is valid in this argument from what is tendentious. The professional science of the twentieth century is vastly different from the natural philosophy of the eighteenth. The divide between the 'learned' and the popular view of the world has widened until, in many cases, there is no community between them. This transformation began in the nineteenth century as a consequence of the professionalisation of science.

However, the recognition of the greater relative autonomy of scientific discourse in the last 150 years does not enforce the claim that science is *ipso facto* 'context independent'. It does suggest that different explanatory strategies may be appropriate to accounts of cosmology in this later period. The notion of science as organised into 'research schools', whose formation and maintenance depend upon a continuous negotiation with other social structures, provides a serviceable resource here.

Studies of modern science have shown the constant labour that the maintenance of reality demands. They have also revealed the extent to which the shape of that reality depends on the interests of those who construct it. The professional competences of scientists and the wider strategies to which they are committed impinge on why they make out the world in certain ways. Moreover, even now when science is a well-entrenched institution, it cannot be oblivious to the wishes of its patrons; the interests of government and industry must be considered, if only in the preparation of research proposals. The fact that science must still compete for limited resources against other claimants alone ensures that it is not immune from the exigencies of politics.

Returning to the nineteenth century, the signs of the dependency of science and of its political aspect become still more apparent. The way in which the need of physiology to gain the support of medicine impinged upon concepts of the organism has been described; the fact that even in the field of comparative embryology a similar concern existed has been noted. Further consideration of these and similar client-patron relations offers the means of uniting two forms of enquiry in the history of science which have previously been separate. On the one hand, 'institutional' history has documented the material growth of schools; on the other, 'intellectual' history has considered the kind of science it produced. If it is recognised that the two are inseparable — that the ideas of a school develop through its material relationships — then the two projects become one.

On this view, professionalisation, far from relieving the pressure of 'society' on science, intensified it. While the gentleman-scientist of 1800 had the means to do as he pleased, the professional schools of 1900 normally operated on a scale beyond the means of any individual to maintain: they were obliged to go out and to solicit support. The scientist had ceased to be a gentleman and had become an entrepreneur.

However, both the amateur and the professional scientist were also creatures of a larger social structure. It has been argued that one of the most obvious idioms of early Victorian science, natural theology, expressed the interests of the conservative elements of society. Did the science of a later era similarly bear an ideological burden? A prevalent trend in contemporary Marxism answers this question in the affirmative: science is presented as the unique embodiment of the presumptions and biases of capitalism. Such analyses tend to concentrate on some very general feature of the 'scientific' world-view,

such as the tendency to abstraction and quantification or an instrumentalist approach to phenomena, and to show how this arose through or is maintained by the peculiar conditions of capitalism. Conceptions of reality of this sort, once 'found' in nature, are then transferred to society to legitimate the current distribution of power.

There is a resemblance between this argument and much of what has been said above about the social uses of nature. In particular, the form of legitimation identified bears an obvious relation to the strategies described under the heading 'The Constraints of Nature' in Chapter Six. There the way in which naturalism became embodied in institutional practice, grew into an implicit rather than an explicit ideology, was mentioned. Recently, there has been some indication that naturalistic formulations are again emerging in contemporary political disputes. Both the issues at stake in, for example, the debates over racial endowment and the kind of resources that are brought to them have much in common with the context which produced the eugenic movement in early twentieth century Britain. The extent of this continuity, as well as the apparent resurgence of naturalism as a polemical weapon in the last two decades, deserve fuller consideration.

But there is a distinction between the 'laws of heredity', as expounded in public debates and in the deliberations of governmental agencies, and what happens at the core of contemporary genetics. There is some relation between the two forms of discourse; both are 'naturalistic' in some sense. However, it is not obvious that both are ideological. Throughout this thesis the notion of *use* has been held to be central to an understanding of the social meaning of cosmologies. It is difficult to see how the fine detail of modern biology could ever be put to use as legitimations if only because of their unintelligibility to all but a handful of individuals. Nor does it seem plausible to suggest that these intricacies merely provide a background of disinterested enquiry to which overtly ideological formulations can be referred.

Polemical use is not the only way in which science can serve social interests. Science, in some of its forms, directly services the military and economic needs of the state. However, an understanding of how this circumstance affects the content of science once again requires analysis in terms of the relation between patron and client. It may well prove possible to establish connections between parts of modern science and its political role; but general statements about the inherently inegalitarian or authoritarian

character of the scientific outlook contribute little to this enquiry.

Similarly, it is not enough to say that Darwinism embodied a normative interpretation of both nature and society favourable to the bourgeoisie without establishing the context of use in which this moral order was employed. There is no necessity that natural knowledge will encode such meanings; certain cosmologies, such as the morphological doctrines mentioned in Chapter Five, are devised for in-group consumption only and articulate no wider interests. Other formulations occupy a more uncertain status. Notably, the theory of the organism as an originally homogeneous mass whose functions are gradually delegated to specialised organs bears a clear relation to both the theory and the reality of the division of labour in Victorian society. That the body natural was used to justify certain aspects of the body politic through this homology is certain; however, it is also certain that this model of the organism served more specialised interests within the biological community. How these two kinds of interest coexisted, and whether either can be regarded as subordinate to the other is unclear. The general question of how the relation between contexts of use should be conceived remains open.

## APPENDIX A: The Huxley Circle

Thomas Huxley was an especially important figure in the naturalist movement. He appears in every chapter of this thesis in a variety of guises; in fact, Huxley was active in virtually every context where naturalistic ideas were deployed. In consequence, his circle of acquaintances included most of the other major personalities in these efforts. This network is represented in figure one.

Like all classifications this rendering of the Huxley Circle is determined by the interests brought to it. It does not claim to be comprehensive; if it were, then each class would have to be considerably expanded. Further, both the classes and the distribution of individuals between them are open to revision. Lubbock, for example, could as well be ranked among the 'Liberal Thinkers' or with 'The Biologists'. But the figure is intended only to indicate the scope of the social movement of which scientific naturalism was a part and to show some of the main actors.

The names of certain of these individuals recur throughout this thesis; it is convenient therefore to provide biographical information on them here. Others who do not appear again are nonetheless of interest in assigning a character to the movement under consideration.

**Brodie, Benjamin Collins (1817 - 80):** Son of an eminent physician. Educated at Harrow and Balliol Oxford where he developed an interest in chemistry. He became Professor of Chemistry at Oxford in 1865

**Carpenter, William Benjamin (1813 - 1885):** Son of a schoolmaster. Wished to be a civil engineer; instead apprenticed to a physician. Entered University College London in 1833 as a medical student. In 1835 went to Edinburgh to study physiology. He lectured for a time in Medical Jurisprudence at Bristol Medical School, and became Fullerian Professor of Physiology at the Royal Institution in London in 1844. In the same year he was elected FRS. After a period as lecturer in physiology at the London Hospital and as Professor of Forensic Medicine at University College, Carpenter became Registrar of London University in 1856. He was active in the politics of this institution throughout his life.

**Clifford, William Kingdom (1845 - 79):** Son of a prominent citizen of Exeter. Educated at King's College London and in 1863 entered Trinity College Cambridge. He distinguished himself in mathematics and was elected to a fellowship at Trinity in 1868. While at Cambridge Clifford was intimate with radicals like Henry Fawcett and was for a time Secretary of the Republican Club. In 1871 Clifford was appointed Professor of Applied Mathematics at University College London. In 1874 he was elected FRS.

**Fawcett, Henry (1833 - 84):** Son of a draper. His father was active in support of the 1831 Reform Act and in the Anti-Corn Law League. Fawcett was educated at the Owenite Queenwood College from 1847; there he studied chemistry and surveying. He went to Cambridge in 1852; he was a member of the 'Mill' or radical party at the university. He helped to secure the abolition of the celibacy condition for the holding of fellowships in 1859. In 1863 he became Professor of Political Economy at Cambridge and acted as a political disciple of Mill. Fawcett stood unsuccessfully for the Parliamentary seats at Cambridge (1863) and Brighton (1864); he was finally elected to the latter constituency in 1865. In Parliament he was a leading radical who helped pass the 1867 Reform Act. He was critical of Gladstone's concessions to the clergy on education.

**Flower, William Henry (1831 - 99):** Son of a brewer and banker of note. Flower was educated at University College London where he studied medicine. Upon graduation he became assistant surgeon and lecturer at Middlesex Hospital. From 1861 - 4 he was Curator of the Hunterian Museum at the Royal College of Surgeons and began work on Comparative Anatomy. In 1870 he succeeded Huxley as Professor of Comparative Anatomy and Physiology at the Royal College of Surgeons. Flower was a leading figure in the Royal Society in the 1860s and 1870s, and became director of the Natural History Museum upon Richard Owen's retirement. He took Huxley's part in the controversy with Owen over the primate brain in 1862.

FIGURE 1

**The Biologists**

M. Foster (1836 - 1907)  
W.H. Flower (1831 - 99)  
E.R. Lankester (1847 - 1929)  
W.K. Parker (1823 - 90)

**The Liberal Thinkers**

W.K. Clifford (1845 - 79)  
H. Fawcett (1833 - 84)  
J. Morley (1838 - 1923)  
F. Pollock (1845 - 1937)  
H. Spencer (1820 - 1903)  
L. Stephen (1832 - 1904)

T.H. Huxley  
(1825 - 95)

**Guilty Bystanders**

C.R. Darwin (1809 - 82)  
C. Lyell (1797 - 1875)

**The Politicians and Bureaucrats of Science**

B.J. Brodie (1817 - 80)  
W.B. Carpenter (1813 - 85)  
E. Frankland (1825 - 99)  
T.A. Hirst (1830 - 92)  
J.D. Hooker (1817 - 1911)  
J.N. Lockyer (1836 - 1920)  
J. Lubbock ( 1834 - 1913)  
L. Playfair (1818 - 98)  
H.E. Roscoe (1833 - 1915)  
J. Tyndall (1820 - 93)



**Foster, Michael (1836 - 1907):** Son of a surgeon. He was educated in the University of London; Foster was unable to go to Cambridge because of the religious tests (his family was Baptist). After studying medicine at University College and in Paris, Foster began practice in 1861. In 1867 he became lecturer in practical physiology at University College. In 1868 he was appointed Professor in the same subject and succeeded Huxley as Fullerian Professor of Physiology at the Royal Institution. In 1870 Foster went to Cambridge as Praelector in Physiology at Trinity College. In 1872 he was elected FRS; nine years later he took over from Huxley as Biology Secretary of the Royal Society. Foster was prominent in spreading Huxley's ideas on laboratory instruction and published 'practical' textbooks of physiology (1873) and embryology (1874). He helped found the Physiological Society in 1875. Foster was concerned to strengthen the links between science and government; he pursued this end especially through his position within the Royal Society. In 1900 he was the Liberal Unionist MP for London University.

**Frankland, Edward (1825 - 99):** Began his career as an apprentice chemist. In 1845 he went to the Museum of Practical Geology in London; there he studied under Lyon Playfair. Frankland taught at Queenwood College from 1847 together with John Tyndall. The two went to Marburg in 1848 for scientific education; Frankland received his PhD in 1849 and went on to work under Liebig at Giessen. In 1850 he was appointed Professor of Chemistry at Pultney College of Civil Engineering; in the following year he secured an appointment at Owen's College in Manchester. He was elected FRS in 1853. Frankland came to London in the 1860s; after a period lecturing at the Royal Institution and at St Bartholemew's Hospital, he became Professor of Chemistry at the Royal College of Mines; he held this post until 1885. Frankland was a member of the 'X-Club': the so-called 'Cabinet' of Victorian science.

**Hirst, Thomas Archer (1830 - 92):** Hirst met Tyndall in 1846 when both were working as surveyors. He went to Marburg in 1849; there he obtained a PhD. He subsequently studied at Göttingen and in Berlin. In 1853 he succeeded Tyndall as teacher at Queenwood. Seven years later he became Master in Mathematics at University College School in London and in 1865 Professor of Physics at University College itself. In 1867 he was translated into the Professor of Pure Mathematics. Hirst was a member of the X-Club.

**Hooker, Joseph Dalton (1817 - 1911):** Son of the Regius Professor of Botany at Glasgow who was also Director of Kew Gardens. Hooker studied medicine at Glasgow and became an MD in 1839. In 1865 he succeeded his father as Director at Kew. He became President of the Royal Society in 1873 and devoted himself to raising extra funds for science. Hooker was a member of the X-Club.

**Huxley, Thomas Henry (1825 - 95):** Son of a schoolmaster. He wished to become a mechanical engineer but settled for medicine. He was a medical apprentice in London and matriculated at London University in 1842. Huxley acquired his knowledge of biology while pursuing advanced medical studies at Charing Cross Hospital. He served as assistant surgeon on *HMS Rattlesnake*, and in that capacity went on a voyage to the South Pacific. On his return Huxley was elected FRS in 1851. In 1854 he was appointed lecturer at the Royal School of Mines and naturalist to the geological survey. He was Hunterian Professor at the Royal College of Surgeons (1863 - 9) and Fullerian Professor at the Royal Institution (1863 - 7). Huxley was deeply involved in the workings of the Royal, Zoological, Geological and Ethnological Societies. He was Secretary to the Royal Society from 1871 and President from 1881 - 5. In addition, Huxley served on various government bodies and as examiner for London University; in 1890 he became the first Dean of the new College of Science in South Kensington. Huxley was a member of the X-Club.

**Lankester, Edwin Ray (1847 - 1929):** Son of a physician and scientist who helped found the *Quarterly Journal of Microscopical Study*. Educated at St Paul's School and Downing College Cambridge. Moved to Oxford where he took Honours in Natural History. Lankester continued his studies in Vienna and Leipzig and at the marine biology station in Naples from 1871 - 2. In 1872 he became fellow and tutor at Exeter College Oxford; two years later he was appointed Professor of Zoology at University College London. Lankester was elected FRS in 1875. In 1884 he helped to found the Marine Biology

Association, and in 1891 was appointed Professor of Comparative Anatomy at Oxford.

**Lockyer, Joseph Norman (1836 - 1920):** Son of a physician. He was educated privately and at 21 became a clerk in the War Office. Lockyer acquired a taste for science from his father and became an active astronomer. In 1875 he was transferred from the War Office to the Science and Art Department and helped establish the Science Museum at South Kensington. In 1890 Lockyer was appointed Professor of Astronomical Physics at the new Royal College of Science. He was editor of *Nature* from 1869.

**Lubbock, John (1834 - 1913):** Son of a baronet and banker who was also Secretary of the Royal Society and a friend of Charles Darwin. Lubbock was educated at Eton and took over the family business at an early age. He learned science in his spare time, with Darwin's help. Lubbock's most distinguished scientific work lay in entomology. In addition to his banking career Lubbock was Liberal MP from 1870, serving first as Member for Maidstone and then for London University. He was Chairman of the Society for the Extension of University Education (1894 - 1902) and Vice-Chancellor of London University (1872 - 80). He also served on the Council of the Royal Society. Lubbock declared himself a Unionist during the home rule crisis. His religious views were unorthodox. His association with Huxley was of long standing: the latter secured Lubbock's election to the Royal Society. Lubbock was a member of the X-Club.

**Morley, John (1838 - 1923):** Son of a surgeon. Educated at Lincoln College Oxford. He worked as a journalist and became involved in radical literary circles in London. Morley was a founder and editor of the *Fortnightly Review* — the main organ for the expression of 'advanced' political, religious and philosophical views during the 1860s and 1870s. Together with Joseph Chamberlain and Henry Dilke, Morley formed a radical triumvirate dedicated to the disestablishment of the Anglican Church, secular education, land reform and progressive taxation.

**Parker, William Kitchen (1823 - 90):** Son of a farmer. Parker was educated at parish and grammar schools and then apprenticed to a druggist. In 1844 he came to London to study medicine; he was assistant to Robert Todd at Charing Cross Hospital. He became a general practitioner and continued medical practice until 1883. In addition, Parker pursued work on vertebrate development; Huxley was his 'chief scientific friend', and much of Parker's research was directed to confirming Huxley's skull theory. He was elected FRS in 1865 and was Hunterian Professor of Comparative Anatomy at the Royal College of Surgeons in 1873.

**Playfair, Lyon (1818 - 98):** Son of the Chief-Inspector-General of hospitals in Bengal. Playfair was educated at St Andrews University. He studied medicine at Glasgow and Edinburgh but did not qualify. He worked with Liebig at Giessen (1839 - 40) and obtained a PhD. From 1842 - 5 he was Honorary Professor of Chemistry at the Royal Institution in Manchester; in 1845 he was appointed chemist to the Geological Survey and professor in the School of Mines. Playfair was elected FRS in 1848. He was President of the Chemical Society from 1857 - 9 and in 1853 became Secretary for Science at the new Department of Science and Art. From 1858 - 69 Playfair was Professor of Chemistry at Edinburgh. In 1868 he was elected as Liberal MP for Edinburgh and St Andrews Universities; from 1885 - 92 he was Member for South Leeds.

**Pollock, Frederick (1845 - 1937):** Son of a baronet. Pollock was educated at Eton and Trinity College Cambridge; in 1868 he was elected a fellow of that College. At Cambridge he was familiar with Clifford, Leslie Stephen and other radicals. He proceeded to the Bar and became Professor of Jurisprudence at Oxford in 1883. He wrote on philosophy throughout his life; especially notable was his book on Spinoza (1880).

**Roscoe, Henry Enfield (1833 - 1915):** Son of a barrister. Studied in London and then with Bunsen in Heidelberg; there he obtained a PhD. Roscoe worked for a time as a part-time teacher and government adviser. He replaced Frankland as Professor of Chemistry at Owen's College in 1857.

**Spencer**, Herbert (1820 - 1903): Son of a school teacher. Spencer's family had strong radical nonconformist links. Spencer was privately educated and worked as a teacher and engineer. He was associated with the 'complete suffrage' movement, which was closely connected with Chartism, in the 1840s. Spencer was described as 'radical all over'. He produced a vastly popular 'Synthetic Philosophy', intended to give an evolutionary interpretation of nature, as well as numerous works on politics and education. Spencer was a member of the X-Club.

**Stephen**, Leslie (1832 - 1904): Son of James Stephen, a Colonial Under-Secretary. Stephen was educated at Eton, King's College London and Trinity Hall Cambridge. He became a fellow of his College and took holy orders in 1855. However, upon 'losing his faith' after a reading of Mill, Comte and Kant, Stephen gave up his clerical career. He became a radical and an advocate of university reform working with others, such as Fawcett, in Cambridge, of similar persuasion. He went to London in 1864 and through his books and articles quickly established himself as a leader of the agnostic school. He was familiar with Huxley, Tyndall and other advocates of scientific naturalism. Stephen's **History of English Thought in the Eighteenth Century** was an exposition of the ideas of the Deists and of Hume.

**Tyndall**, John (1820 - 93): Son of a leather-worker. Tyndall was mainly self-educated. He worked on the ordnance survey and as railway engineer. He taught at Queenwood College from 1847 - 8 before going to Marburg to study physics and chemistry. Tyndall was Professor of Natural Philosophy at the Royal Institution from 1853 and in 1867 became its Superintendent. In 1859 he was appointed Professor of Physics at the Royal College of Mines. Tyndall was poisoned by his wife (accidentally). He was a member of the X-Club.

## APPENDIX B: Glossary of Zoological Terms Used in Chapter Five

**Actinians:** A genus of polyp characterised by many arms radiating around the mouth.

**Amphioxus:** A marine creature, apparently intermediate between worms and fish, which possesses a rudimentary vertebral column.

**Articulata (Arthropods):** Class of animals with external jointed skeletons or jointed limbs.

**Ascidians:** Shell-less acephalous molluscs such as the sea-squirt.

**Brachiopoda:** A class of acephalous molluscs distinguished by two spiral arms proceeding from the mouth.

**Chaetopods:** A class containing most of the segmented worms (Annelids).

**Ctenophores:** A group of jellyfish separated from Coelentrata because of the shape of their bell.

**Diphyidae:** A family of Hydrozoa with a pair of swimming bells opposite each other on the upper part of the body.

**Echinoderms:** The class of radially-symmetrical animals most of which have spiky skins.

**Entomostraca:** An order of Crustacea often enclosed in a bivalve shell.

**Ganoid:** Any fish with bony scales covered with 'ganoin' — a shiny enamel-like substance.

**Heteropoda:** A group of Molluscs whose ventral foot is compressed into a fin.

**Hydrozoa:** A group of simple, usually colonial polyps.

**Medusa:** A group of jellyfish with a bell-like disk.

**Monostome Medusae:** Those members of the order with simple mouths.

**Myxinoid:** A genus of jawless fish also known as slime eels.

**Phyllopoda:** A group of Crustacea with bivalve shells and at least four pairs of leaf-like swimming feet.

**Physophoridae:** A family of Hydrozoa whose members float by means of vesicular organs.

**Polyps:** A simple animal with a cylindrical body with the mouth at one end surrounded by tentacles.

**Pteropoda:** A class of Mollusc whose limbs are wing-shaped.

**Rhizostome Medusae:** Those members of the order characterised by secondary mouths located on stalks.

**Vermes:** Unsegmented worms.

## BIBLIOGRAPHY

## Notes

The following abbreviations are used below.

AS: Annals of science

BHM: Bulletin of the history of medicine

BJHS: British journal for the history of science

CR: Contemporary review

FR: Fortnightly review

HS: History of science

I: Isis

JHB: Journal of the history of biology

JHI: Journal of the history of ideas

JMS: Journal of mental science

N: Nature

NC: Nineteenth century

QJMS: Quarterly journal of microscopical science

QR: Quarterly review

Place of publication is London unless otherwise stated.

## 1. MANUSCRIPT SOURCES

A.J. Balfour Papers: British Museum, London.

G. Combe Papers: National Library of Scotland, Edinburgh.

A. Seth Pringle-Pattison Papers: *ibid.*

Thomas Laycock Papers: University Library, Edinburgh.

John Goodsir Papers: *ibid.*

T.H. Huxley Papers: Imperial College, London.

Darwin-Lyell Correspondence: Library of the American Philosophical Society, Philadelphia.

Owen Papers: *ibid.*

I wish to thank the Librarians of these institutions for their assistance.

## 2. PUBLISHED SOURCES

## A.H.G.

1874 ..... 'Foster and Balfour's "Embryology"', N, 11, 126 - 8.

## ABBOTT, E. and L. CAMPBELL

1892 ..... The life and letters of Benjamin Jowett, 2 vols, John Murray.

## ABERNETHY, J.

1815 ..... Part of the introductory lecture for the year 1815, exhibiting some of Mr Hunter's opinions respecting diseases, Longmans.

1817 ..... Physiological lectures, exhibiting a general view of Mr Hunter's physiology, and of his researches into comparative anatomy, Longmans.

1821 ..... Reflections on Gall and Spurzheim's physiognomy and phrenology, Longmans.

## ACKERKNECHT, E. H.

1974 ..... 'The history of the discovery of the vegetative (autonomic) nervous system', *Medical history*, 18, 1 - 8.

## ADAMSON, J.W.

1930 ..... *English education 1789 - 1902*, Cambridge: Cambridge University Press.

## ADDISON, A.

1863 ..... 'On the pathological anatomy of the brain in insanity', *JMS*, 8, 37 - 61.

## ALBURY, W.R.

1977 ..... 'Experiment and explanation in the physiology of Bichat and Magendie', *Studies in the history of biology*, 1, 47 - 131.

## ALISON, W.P.

1839 ..... *Outlines of human physiology*, 3rd ed., Edinburgh: William Blackwood.

## ANON.

1864 ..... 'The Edinburgh and the Quarterly on the new theology', *The reader*, 4, 123 - 4.

1864a ..... 'Review of M. Verdet "Exposé de la theorie mécanique de la chaleur"', and J. Tyndall,

"Heat considered as a mode of motion" ', *North British review*, 40, 40 - 69.

1865 ..... 'British and foreign science', *The reader*, 6, 61 - 2.

1870 - 1 ..... [M. Foster's physiology lectures at Trinity College Cambridge, 1870], *N*, 3, 73 - 4.

1884 ..... 'Marine biological association of the United Kingdom', *N*, 30, 40, 82, 275, 323, 350.

#### ANSCHUTZ, R.P.

1953 ..... *The philosophy of J.S. Mill*, Oxford: Clarendon Press.

#### APPEL, A.T.

1975 ..... *The Cuvier-Geoffroy debate and the structure of nineteenth century French zoology*, Princeton University PhD.

#### ARGYLL, Duke of

1867 ..... *The reign of law*, Alexander Strahan.

#### ARLIDGE, J.T.

1859 ..... *On the state of lunacy and the legal provision for the insane, with observations on the construction and organization of asylums*, John Churchill.

#### BAAS.

1867 ..... 'Report of the committee appointed by the Council of BAAS to consider the best means for promoting scientific education in schools', *Report of the British Association for the Advancement of Science (Dundee)*, xxxix-liv.

#### BABBAGE, C.

1851 ..... *The exposition of 1851; or, views of the industry, the science, and the government, of England*, John Murray.

#### BAIN, A.

1855 ..... *The senses and the intellect*, John Parker.

1859 ..... *The emotions and the will*, John Parker.

1867 ..... 'On the correlation of force and its bearing on mind', *Macmillans magazine*, 16, 372 - 83.

#### BAKER, J.R.

1948 - 53 ..... 'The cell-theory: a restatement, history and critique', *QJMS*, 89, 103 - 23; 90, 87 - 108; 93, 157 - 90; 94, 407 - 44.



**BALFOUR, A.J.**

- 1879 ..... *A defence of philosophic doubt*, Macmillan.
- 1893 ..... *Essays and addresses*, Edinburgh: David Douglas.
- 1895 ..... *The foundations of belief: being notes introductory to the study of theology*, Longman.
- 1904 ..... *Reflections suggested by the new theory of matter*, Longman.
- 1930 ..... *Chapters of autobiography* (ed. E. Dugdale), Cassell.

**BALFOUR, F.M.**

- 1875 ..... 'A comparison of the early stages in the development of vertebrates', *QJMS*, new series, **15**, 207 - 26.
- 1880 ..... 'On the structure and homologies of the germinal layers of the embryo', *QJMS*, new series, **20**, 247 - 73.
- 1885 ..... *A treatise of comparative embryology*, 2 vols, Macmillan.

**BANNISTER, R.C.**

- 1979 ..... *Social Darwinism: science and myth in Anglo-American social thought*, Philadelphia: Temple University Press.

**BARCLAY, J.**

- 1822 ..... *An inquiry into the opinions, ancient and modern, concerning life and organization*, Edinburgh: Balfour and Clarke.

**BARNES, B.**

- 1977 ..... *Interests and the growth of knowledge*, Routledge.

**BARNES, B. and S. SHAPIN**

- 1979 ..... *Natural order: historical studies of scientific culture*, Beverly Hills: Sage.

**BARRY, A.**

- 1869 ..... 'The battle of the philosophies — physical and metaphysical', *CR*, **12**, 232 - 44.

**BARRY, M.**

- 1837 ..... 'On the unity of structure in the animal kingdom', *Edinburgh new philosophical journal*, **22**, 116 - 41.
- 1837a ..... 'Further observations on the unity of structure in the animal kingdom ..... with some remarks on embryology, as facilitating animal nomenclature, classification, and the study of comparative anatomy', *Edinburgh new philosophical journal*, **22**, 345 - 64.

**BARRY, W.F.**

1891 ..... 'Ethics of the day', *QR*, 172, 65 - 95.

**BARTHOLEMEW, M.**

1975 ..... 'Huxley's defence of Darwin', *AS*, 32, 525 - 35.

**BASALLA, G. et al. (eds.)**

1970 ..... *Victorian science a self-portrait from the presidential addresses of the British Association for the Advancement of Science*, Bell.

**BASTIAN, H.C.**

1870 ..... 'Protoplasm', *N*, 1, 424 - 6.

**BEALE, L.**

1856 ..... *On some points of the anatomy of the liver*, John Churchill.

1857 ..... *How to work with the microscope*, John Churchill.

1861 ..... *On the structure of the simplest tissues of the human body*, John Churchill.

1865 ..... *On the structure and growth of the tissues, and on life*, Robert Hardwicke.

1871 ..... *Life theories: their influence upon religious thought*, John Churchill.

1874 ..... *Protoplasm: or, matter and life*, John Churchill.

1899 ..... *Vitality: an appeal to the fellows of the Royal Society*, John Churchill.

**BÉCLARD, P.A.**

1821 ..... *Additions à l'anatomie general de Xav. Bichat*, Paris: J.A. Brosson.

**BELL, C.**

1836 ..... *The nervous system of the human body*, 3rd ed., Edinburgh: Adam and Charles.

1852 ..... *The hand; its mechanism and vital endowments as evincing design*, 5th ed., John Murray.

1966 ..... *Idea of a new anatomy of the brain (1811)*, facsimile ed., Dawsons.

**BENTON, E.**

1974 ..... 'Vitalism in nineteenth-century thought: a typology and reassessment', *Studies in the history and philosophy of science*, 5, 17 - 48.

**BEST, G.**

1971 ..... *Mid-Victorian Britain*, Weidenfeld and Nicolson.

**BICHAT, X.**

1805 ..... *Recherches physiologiques sur la vie et la mort*, 3rd ed., Paris: Brosson.

1812 ..... *Anatomie generale, appliqué a physiologie et a la médecine*, 2nd ed., Paris: Brosson.

**BLANDFORD, G.F.**

1871 ..... *Insanity and its treatment: lectures on the treatment, medical and legal, of insane patients*, Edinburgh: Oliver and Boyd.

**BOOTH, C.**

1886 ..... 'Occupation of the people of the United Kingdom, 1801 - 81', *Journal of the Royal Statistical Society*, 49, 314 - 435.

**BORING, E.G.**

1929 ..... *A history of experimental psychology*, New York: Appleton - Century - Crofts.

**BOSANQUET, B.**

1906 ..... *The meaning of teleology*, H. Frowde.

**BOWDITCH, H.P.**

1870 - 1 ..... 'The physiological laboratory at Leipzig', *N*, 3, 142 - 3.

**BOWMAN, W.**

1892 ..... *Collected papers* (ed. J. Burdon Sanderson and J.W. Hulke), 2 vols, Harrison and Son.

**BROCK, W.H.**

1966 ..... 'The selection of the authors of the Bridgewater Treatises', *Notes and records of the Royal Society*, 21, 162 - 79.

**BROCK, W.H. and R.M. MACLEOD**

1976 ..... 'The scientists' declaration: reflexions on science and belief in the wake of *Essays and reviews*, 1864 - 5', *BJHS*, 9, 39 - 66.

**BRODIE, B.C.**

1872 - 3 ..... 'Scientific research and university endowments', **N**, 7, 97 - 8.

**BROOKE, J.H.**

1977 ..... 'Natural theology and the plurality of the worlds: observations on the Brewster-Whewell debate', **AS**, 34, 221 - 86.

1977a ..... 'Richard Owen, William Whewell, and the Vestiges', **BJHS**, 10, 132 - 45.

**BROWN, G.H.**

1955 ..... **Lives of the fellows of the Royal College of Physicians of London 1826 - 1925**, Royal College of Physicians.

**BROWN, K.D. (ed.)**

1974 ..... **Essays in anti-labour history: responses to the rise of labour in Britain**, Macmillan.

**BROWNE, W.A.F.**

1864 ..... 'The moral treatment of the insane', **JMS**, 10, 309 - 37.

1866 ..... 'Address on medico-psychology', **JMS**, 12, 309 - 27.

**BRUSH, S.G.**

1967 ..... 'Thermodynamics and history', **Graduate journal**, 7, 477 - 565.

1976 ..... **The kind of motion we call heat: a history of the kinetic theory of gases in the 19th century**, 2 vols, Amsterdam/New York: North Holland Publishing Co.

**BUCKLE, H.T.**

1858 - 61 ..... **History of civilization in England**, 2 vols, John Parker.

**BUCKNILL, J.C.**

1856 ..... 'Review of Thomas Laycock's "Further researches of the functions of the brain"', **JMS**, 2, 175 - 88.

1857 ..... 'Review of "The treatment of the insane without mechanical restraints" by John Conolly', **JMS**, 3, 253 - 61.

**BUCKNILL, J.C. and D.H. TUKE**

1862 ..... **A manual of psychological medicine: containing the history, nosology, description, statistics, diagnosis, pathology, and treatment of insanity**, 2nd ed., John Churchill.

**BURDON SANDERSON, G.**

1911 ..... Sir John Burdon Sanderson: a memoir, Oxford: Clarendon Press.

**BURDON SANDERSON, J.**

1870 - 1 ..... 'Physiological laboratories in Great Britain', *N*, **3**, 189.

1879 ..... Syllabus of a course of lectures on physiology, 2nd ed., H.K. Lewis.

**BURGESS, J.**

1858 ..... The medical and legal relations of madness: showing a cellular theory of mind, and of nerve force, and also of vegetative and vital force, John Churchill.

**BURROW, J.W.**

1966 ..... Evolution and society: a study in Victorian social theory, Cambridge: Cambridge University Press.

**BUTT, J. and I.F. CLARKE (eds.)**

1973 ..... Victorians and social protest, David and Charles.

**BYNUM, W.F.**

1964 ..... 'Rationales for therapy in British psychiatry: 1780 - 1835', *Medical history*, **18**, 317 - 33.

**CAMPBELL, L. and W. GARNETT**

1882 ..... The life of James Clerk Maxwell, Macmillan.

**CANNON, S.F.**

1978 ..... Science in culture: the early Victorian period, New York: Dawson.

**CARDWELL, D.S.L.**

1972 ..... The organisation of science in England, Heinemann.

**CARMICHAEL, A.**

1830 ..... An essay, on such physical considerations as are connected with man's ultimate destinations, the essential constitution of superior beings; and the presumptive unity of nature, Dublin: Shaw.

**CARPENTER, W.B.**

1837 - 8 ..... 'On the differences of the laws regulating vital and physical forces', *Edinburgh new philosophical journal*, 24, 327 - 53.

1839 ..... *Principles of general and comparative anatomy*, John Churchill.

1855a ..... ditto, 4th ed.

1842 ..... *Principles of human physiology, with their chief applications to pathology, hygiene, and forensic medicine*, John Churchill.

1846 ..... ditto, 3rd ed.

1853 ..... ditto, 4th ed.

1855 ..... ditto, 5th ed.

1846a ..... *A manual of physiology, including physiological anatomy for the use of the medical student*, John Churchill.

1856a ..... ditto, 3rd ed.

1847 ..... *Animal physiology: a comprehensive sketch of the principal forms of animal structure*, William. S. Orr.

1859 ..... ditto, 2nd ed.

1856 ..... *The microscope and its revelations*, John Churchill.

**CECIL, H.**

1912 ..... *Conservatism*, Williams and Norgate.

**CLAGETT, M. (ed.)**

1962 ..... *Critical problems in the history of science*, Madison: University of Wisconsin Press.

**CLARKE, E. and C.D. O'MALLEY**

1968 ..... *The human brain and spinal cord: a historical study illustrated by writings from antiquity to the twentieth century*, Berkeley: University of California Press.

**CLIFFORD, W.K.**

1877 ..... 'The influence upon morality of a decline in religious belief', *NC*, 1, 331 - 58.

1878 - 87 ..... *Elements of dynamic: an introduction to the study of motion and rest in solid and fluid bodies*, 2 vols, Macmillan.

1879 ..... *Lectures and essays* (ed. L. Stephen and F. Pollock), Macmillan.

1879a ..... *Seeing and thinking*, Macmillan.

1946 ..... *The common sense of the exact sciences* (ed. K. Pearson), new ed., Sigma Books.

**COLE, G.D.H.**

1954 ..... *A history of socialist thought: vol ii Marxism and anarchism 1850 - 1890*, Macmillan.

**COLEMAN, W.**

1965 ..... 'Cell, nucleus and inheritance: a historical study', *Proceedings of the American Philosophical Society*, 109, 124 - 58.

1976 ..... 'Morphology between type concept and descent theory', *Journal of the history of medicine*, 31, 149 - 75.

1977 ..... *Biology in the nineteenth century: problems of form, function and transformation*, 2nd ed., Cambridge: Cambridge University Press.

**COMBE, A.**

1831 ..... *Observations on mental derangement: being an application of the principles of phrenology to the elucidation of the causes, symptoms, nature and treatment of insanity*, Edinburgh: John Anderson.

**COMBE, G.**

1845 ..... *Elements of phrenology*, 6th ed., Edinburgh: Maclachlan and Stewart.

**COMMISSION**

1864 ..... *Report of the Royal Commission into public schools and colleges*, HMSO.

1874 ..... *Report of the Royal Commission on scientific instruction and the advancement of science*, 2 vols, HMSO.

1874a ..... *Report of the Commissioners appointed to inquire into the property and income of the Universities of Oxford and Cambridge*, HMSO.

**CONWAY, M.D.**

1904 ..... *Autobiography: memories and experience of Moncure Daniel Conway*, 2 vols, Cassell.

**COOTER, R.J.**

1976 ..... 'Phrenology: the provocation of progress', *HS*, 14, 211 - 34.

1976a ..... 'Phrenology and British alienists, c. 1825 - 1845', *Medical history*, 20, 1 - 21, 135 - 51.

1978 ..... *The cultural meaning of popular science: phrenology and the organization of consent in nineteenth-century Britain*, Cambridge PhD.

**CRACKANTHORPE, M.**

1908 ..... 'Eugenics as a social force', *NC*, 63, 962 - 72.

**CRANFIELD, P.F.**

1974 ..... *The way in and the way out: Francois Magendie, Charles Bell, and the roots of the spinal nerves*, Mount Kisco: Futura.

**CROOM ROBERTSON, G.**

1876 ..... 'Prefatory words', *Mind*, 1, 1 - 6.

1883 ..... 'Psychology and philosophy', *Mind*, 8, 1 - 21.

## CROSSLEY, L.M.

1979 ..... The professionalisation of science in Victorian Britain, New South Wales PhD.

## CURTIS, L.P.

1963 ..... Coercion and conciliation in Ireland, 1880 - 1892, Princeton: Princeton University Press.

## CUVIER, G.

1859 ..... The animal kingdom, arranged according to its organization (trans. E. Blyth), A. Fullarton.

## DANGERFIELD, G.

1966 ..... The strange death of Liberal England, Macgibbon and Kee.

## DARWIN, C.R.

1901 ..... The descent of man and selection in relation to sex (1871), new ed., John Murray.

1958 ..... The autobiography of Charles Darwin 1809 - 1882, Collins.

1965 ..... The expression of emotions in man and animals (1872), new ed., Chicago: University of Chicago Press.

1977 ..... The origin of species by natural selection: or the preservation of favoured races in the struggle for life (1859), new ed., Harmondsworth: Penguin Books.

## DARWIN, E.

1915 ..... Emma Darwin: a century of family letters 1792 - 1896, 2 vols, John Murray.

## DARWIN, F.

1887 ..... The life and letters of Charles Darwin, 3 vols, John Murray.

## DASTON, L.J.

1978 ..... 'British responses to psycho-physiology, 1860 - 1900', I, 69, 192 - 208.

## DAVEY, J.G.

1853 ..... On the nature, and proximate causes, of insanity, John Churchill.

## DAVIE, G.E.

1964 ..... The democratic intellect: Scotland and her universities in the nineteenth century, Edinburgh:



Edinburgh University Press.

DAVY, H.

1839 - 40 ..... The collected works of Humphrey Davy (ed. J. Davy), 9 vols, Smith and Elder.

DE BEER, G.R.

1937 ..... The development of the vertebrate skull, Oxford: Clarendon Press.

DE GIUSTINO, D.

1975 ..... Conquest of mind, phrenology and Victorian social thought, Croom Helm Social History Series.

DE LAVELEYE, E.

1883 ..... 'The progress of socialism', CR, 43, 561 - 82.

DORAN, B.G.

1975 ..... 'Origins and consolidation of field theory in nineteenth century Britain: from the mechanical to the electromagnetic view of nature', Historical studies in the physical sciences, 6, 133 - 260.

DRUMMOND, J. and C.B. UPTON

1902 ..... The life and letters of James Martineau, 2 vols, James and Nisbet.

DUGDALE, B.E.C.

1936 ..... Arthur James Balfour, 2 vols, Hutchinson.

DURANT, J.R.

1977 ..... The meaning of evolution: post-Darwinian debates on the significance for man of the theory of evolution, 1858 - 1908, Cambridge PhD.

1979 ..... 'Scientific naturalism and social reform in the thought of Alfred Russel Wallace', BJHS, 12, 31 - 58.

EDWARDS, C.

1894 ..... 'The policy of labour', CR, 66, 269 - 79.

EMY, H.V.

1973 ..... *Liberals, radicals and social politics 1892 - 1914*, Cambridge: Cambridge University Press.

ENGELHARDT, H.T.

1975 ..... 'John Hughlings Jackson and the mind-body relation', *BHM*, 49, 137 - 51.

EUGENICS

1913 ..... *Report of the proceedings of the first eugenics congress*, 2 vols, Eugenics Education Society.

EVE, A.S. and C.H. CREASEY

1945 ..... *Life and work of John Tyndall*, Macmillan.

EVERETT, E.M.

1939 ..... *The party of humanity: the Fortnightly Review and its contributors, 1865 - 1874*, Chapel Hill: University of N. Carolina Press.

EXLEY, T.

1829 ..... *Principles of natural philosophy: or, a new theory of physics, founded on gravitation, and applied in explaining the general properties of matter*, Longman.

FARBER, P.L.

1976 ..... 'The type-concept in zoology during the first half of the nineteenth century', *JHB*, 9, 93 - 119.

FARLEY, J.

1977 ..... *The spontaneous generation controversy from Descartes to Oparin*, Baltimore: Johns Hopkins University Press.

FARRAR, A.S.

1862 ..... *A critical history of free thought in reference to the Christian religion*, John Murray.

1863 ..... 'A critical history of free thought in reference to Christian religion'. *QR*, 116, 59 - 97.

FEARING, F.

1964 ..... *Reflex action: a study in the history of physiological psychology*, Hafner.

**FIGLIO, K.**

1975 ..... 'Theories of perception and the physiology of mind in the late eighteenth century', *HS*, 13, 177 - 212.

**FLETCHER, J.**

1835 ..... *Rudiments of physiology*, 2 vols, Edinburgh: John Carfrae.

**FLOURENS, P.**

1842 ..... *Recherches expérimentales sur les propriétés et les fonctions du système nerveux dans les animaux vertébrés*, 2nd ed., Paris: Ballière.

**FOSTER, M.**

1876 ..... *A course of elementary practical physiology*, Macmillan.

1877 ..... *A text book of physiology*, Macmillan.

**FOSTER, M. and F.M. BALFOUR**

1874 ..... *The elements of embryology*, Macmillan.

**FRASER, T. R.**

1868 ..... 'On the connections between chemical constitution and physiological action', *Journal of anatomy and physiology*, 2, 224 - 42.

**FREEDEN, M.**

1976 ..... 'Biological and evolutionary roots of the new Liberalism in England', *Political theory*, 4, 471 - 90.

1978 ..... *The new Liberalism: an ideology of social reform*, Oxford, Clarendon Press.

1979 ..... 'Eugenics and progressive thought: a study in ideological affinity', *Historical journal*, 22, 645 - 71.

**FRENCH, R.D.**

1971 ..... 'Some problems and sources in the foundations of modern physiology in Great Britain', *HS*, 10, 28 - 55.

**GALL, F.J. and G. SPURZHEIM**

1810 ..... *Anatomie et physiologie du système nerveux en générale, et du cerveau en particulier*, 4 vols, Paris: F. Schoell.

**GARFINKLE, N.**

1955 ..... 'Science and religion in England, 1790 - 1800. The critical response to the work of Erasmus Darwin', *JHI*, **16**, 376 - 88.

**GARRETT, C.**

1973 ..... 'Joseph Priestley, the millennium and the French Revolution', *JHI*, **34**, 51 - 66.

**GASMAN, D.**

1971 ..... **The scientific origins of national socialism: social Darwinism in Ernst Haeckel and the German Monist League**, Macdonald.

**GEGENBAUR, K.**

1859 ..... <sup>W</sup>**Grundzüge der vergleichenden Anatomie**, Leipzig: W. Engelmann.

1878 ..... **Elements of comparative anatomy** (trans. F.J. Bell and E.R. Lankester), Macmillan.

**GEISON, G.L.**

1972 ..... 'Social and institutional factors in the stagnation of English physiology 1840 - 1870', *BHM*, **46**, 30 - 58.

1978 ..... **Michael Foster and the Cambridge school of physiology. The scientific enterprise in late Victorian society**, Princeton: Princeton University Press.

**GELLNER, E.**

1974 ..... **The devil in modern philosophy**, Routledge.

**GHISELIN, M.T.**

1973 ..... 'Darwin and evolutionary psychology', *Science*, **179**, 964 - 8.

**GIDDENS, A.**

1977 ..... **New rules of sociological method: a positive critique of interpretative sociologies**, 2nd ed., Hutchinson.

**GIERE, R. and R.S. WESTFALL**

1973 ..... **Foundations of scientific method: the nineteenth century**, Bloomington: Indiana University Press.

**GILLISPIE, C.C.**

1959 ..... *Genesis and geology: a study in the relations of scientific thought, natural theology, and social opinion in Great Britain, 1790 - 1850*, New York: Harper Torchbooks.

**GLICK, T.F.**

1972 ..... *The comparative reception of Darwinism*, Austin: University of Texas Press.

**GOLDMANN, L.**

1964 ..... *The hidden god* (trans. P. Thody), Routledge.

**GOODFIELD, G.J.**

1960 ..... *The growth of scientific physiology*, Hutchinson.

1969 ..... 'Some aspects of English physiology: 1780 - 1840', *JHB*, 2, 283 - 320.

**GOODSIR, JOHN**

1845 ..... *Anatomical and pathological observations*, Edinburgh: Myles Macphail.

1868 ..... *The anatomical memoirs of John Goodsir* (ed. W. Turner), 2 vols, Edinburgh: Adam and Charles Black.

**GOODSIR, JOSEPH**

1868 ..... *The divine rule proceeds by law*, Williams and Norgate.

1868a ..... *The bases of life: a discourse in reply to Professor Huxley's lecture on "The physical basis of life"*, Williams and Norgate.

**GOULD, S.**

1977 ..... *Ontogeny and phylogeny*, Cambridge Mass: Belknap Press.

**GRAHAM, W.**

1895 ..... 'The collectivist prospect in England', *NC*, 37, 15 - 31.

**GREENBLATT, S.H.**

1977 ..... 'The development of Hughlings Jackson's approach to diseases of the nervous system, 1863 - 1866: unilateral seizure, hemiplegia and aphasia', *BHM*, 51, 412 - 30.

GREENE, J.C.

1977 ..... 'Darwin as social evolutionist', *JHB*, 10, 1 - 27.

GREGORY, F.

1977 ..... *Scientific materialism in nineteenth century Germany*, Dordrecht: Reidel.

GROTE, J.

1865 ..... *Exploratio philosophica*, Cambridge: Deighton and Bell.

1867 ..... 'What is materialism?', *Macmillan's magazine*, 15, 370 - 81.

GROVE, W.R.

1855 ..... *The correlation of physical forces*, 3rd ed., Longman.

GRUBER, H. and P.H. BARRETT

1974 ..... *Darwin on man: a psychological study of scientific creativity*, Wildwood House.

GUSSIN, A.E.S.

1963 ..... 'Jacques Loeb: the man and his tropism theory of animal conduct', *Journal of the history of medicine*, 18, 321 - 36.

HADDON, A.C.

1887 ..... *An introduction to the study of embryology*, Charles and Griffin.

HALDANE, R.B.

1888 ..... 'The Liberal Party and its prospects', *CR*, 53, 145 - 60.

HALÉVY, E.

1934 ..... *The growth of philosophic radicalism*, Faber and Faber.

1951 ..... *Imperialism and the rise of labour*, 2nd ed., Ernest Benn.

1952 ..... *The rule of democracy*, 2nd ed., Ernest Benn.

HALL, C.

1861 ..... *Memoirs of Marshall Hall*, R. Bentley.

**HALL, M.**

- 1837 ..... *Memoirs of the nervous system*, Sherwood and Piper.  
 1843 ..... *New memoir of the nervous system*, Hippolyte Bailliere.  
 1850 ..... *Synopsis of the diastaltic nervous system*, Joseph Mallet.

**HALL, T.S.**

- 1969 ..... *Ideas of life and matter. Studies in the history of general physiology 600 BC - 1900 AD*, 2 vols, Chicago: University of Chicago Press.

**HAMBURGER, J.**

- 1965 ..... *Intellectuals in politics: John Stuart Mill and the philosophical radicals*, New Haven: Yale University Press.

**HAMILTON, W.**

- 1859 ..... *Lectures on metaphysics*, 2 vols, William Blackwood.

**HARVIE, C.**

- 1976 ..... *The lights of Liberalism: university Liberals and the challenge of democracy 1860 - 86*, Allen Lane.

**HEIMANN, P.M.**

- 1972 ..... 'Physics and the philosophy of nature in Victorian Britain', *BJHS*, 6, 73 - 9.  
 1978 ..... 'Voluntarism and immanence: conceptions of nature in eighteenth-century thought', *JHI*, 39, 271 - 83.

**HEIMANN, P.M. and J.E. MCGUIRE**

- 1971 ..... 'Newtonian forces and Lockeian powers: concepts of matter in eighteenth-century thought', *Historical studies in the physical sciences*, 3, 233 - 306.

**HELFAND, M.S.**

- 1977 ..... 'T.H. Huxley's "Evolution and ethics": the politics of evolution and the evolution of politics', *Victorian studies*, 20, 159 - 77.

**HELMHOLTZ, H.**

- 1853 ..... 'On the conservation of force', *Scientific memoirs: natural philosophy*, 1, 114 - 62.  
 1873 ..... *Popular lectures on scientific subjects*, Longmans.

**HERSCHEL, J.**

1833 ..... *A treatise on astronomy*, Longmans.

1857 ..... *Essays from the Edinburgh and Quarterly reviews, with addresses and other pieces*, Longmans.

1866 ..... *Familiar lectures on scientific subjects*, Alexander Strahan.

1966 ..... *A preliminary discourse of the study of natural philosophy* (1830), facsimile ed., Johnson Reprint Corp.

**HILL, K. (ed.)**

1964 ..... *The management of scientists*, Boston: Beacon Press.

**HIRST, F.W.**

1927 ..... *Early life and letters of John Morley*, 2 vols, Macmillan.

**HIS, W.**

1888 ..... 'On the principles of animal morphology', *Proceedings of the Royal Society of Edinburgh*, 15, 287 - 98.

**HOBBS, T.**

1968 ..... *Leviathan* (1651), new ed., Harmondsworth: Penguin Books.

**HOBHOUSE, L.T.**

1904 ..... *Democracy and reaction*, Fischer and Unwin.

**HOBBSAWN, E.J.**

1962 ..... *The age of revolutions: 1789 - 1848*, Weidenfeld and Nicolson.

**HOOD, S.**

1822 ..... *Analytic physiology*, Liverpool: James Smith.

**HOOKE, J.**

1868 ..... 'Presidential address', *Proceedings of the British Association for the Advancement of science* (Norwich), lviii-lxxv.



**HUME, D.**

1817 ..... **A treatise on human nature: being an attempt to introduce the experimental method into moral subjects** (1739 - 40), new ed., 2 vols, Thomas and Joseph Allman.

1927 ..... **Enquiries concerning the human understanding and concerning the principles of morals** (1748), new ed., Oxford: Clarendon Press.

**HUNTER, R. and I. MACALPINE**

1963 ..... **Three hundred years of psychiatry 1535 - 1860: a history presented in selected English texts**, Oxford University Press.

**HURLBUTT, R.H.**

1965 ..... **Hume, Newton, and the design argument**, Lincoln: University of Nebraska Press.

**HUXLEY, L.**

1900 ..... **Life and letters of Thomas Huxley**, 2 vols, Macmillan.

**HUXLEY, T.H.**

1864 ..... **An elementary atlas of comparative osteology**, Williams and Norgate.

1866 ..... **Lessons in elementary physiology**, Macmillan.

1869 ..... 'The scientific aspect of positivism', **FR**, new series, 5, 653 - 70.

1877 ..... **A manual of the anatomy of invertebrated animals**, John Churchill.

1893 - 4 ..... **Collected essays**, 9 vols, Macmillan.

1894 ..... 'Past and present', **N**, 51, 1 - 3.

1895 ..... 'Mr Balfour's attack on agnosticism', **NC**, 37, 527 - 40.

1898 ..... **Scientific memoirs** (ed. E.R. Lankester and M. Foster), 4 vols, Macmillan.

**JACKSON, J.H.**

1875 ..... **Clinical and physiological researches on the nervous system. No. 1 — on the localisation of movements of the brain**, John Churchill.

1931 ..... **Selected writings of John Hughlings Jackson** (ed. J. Taylor), 2 vols, Hodder and Stoughton.

**JACOB, J.R.**

1977 ..... **Robert Boyle and the English revolution**, New York: Burt Franklin.

JACOB M.C.

1976 ..... **The Newtonians and the English revolution**, Hassocks: Harvester Press.

JACYNA, L.S.

1980 ..... 'Science and social order in the thought of A.J. Balfour', **I**, 71, 11 - 34.

JENNINGS, L.J.

1881 ..... 'The revolutionary party', **QR**, 151, 285 - 316.

JONES, K.

1967 ..... **Mental health and social policy 1845 - 1959**, 2nd ed., Routledge.

JONES T.R.

1855 ..... **General outline of the animal kingdom, and manual of comparative anatomy**, 2nd ed., John Van Voorst.

JORDANOVA, L.J. and R.S. PORTER

1979 ..... **Images of the earth: essays in the history of the environmental sciences**, Aberdeen: British Society for the History of Science Monographs.

*Chalfont St. Giles*

KARGON, R.H.

1977 ..... **Science in Victorian Manchester: enterprise and expertise**, Manchester: Manchester University Press.

KAUFMANN, M.

1885 ..... 'Socialism and atheism', **CR**, 47, 823 - 40.

KEBBEL, T.E.

1892 ..... 'Fifty years of Conservative influence', **Blackwood's magazine**, 151, 1 - 21.

KENT, C.B.R.

1899 ..... **The English radicals: a historic sketch**, Longmans.

## KIRBY, W.

1852 ..... On the power, wisdom and goodness of God, as manifested in the creation of animals, and in their history, habits, and instincts (1835), 2nd ed., 2 vols, Henry G. Bohn.

## KNOX, R.

1843 ..... Contributions to anatomy and physiology, Wilson and Ogilvy.

## KOHLE, R.E.

1973 ..... 'The enzyme theory and the origin of biochemistry', I, 64, 181 - 96.

## KÖLLIKER, A.

1853 ..... Manual of human histology (trans. G. Busk and T.H. Huxley), 2 vols, Sydenham Society.

1860 ..... A manual of human microscopic anatomy, John Parker.

## KUHN, T.

1970 ..... The structure of scientific revolutions, 2nd ed., Chicago: Chicago University Press.

## KUYPERS, M.S.

1930 ..... Studies in the eighteenth century background of Hume's empiricism, Minneapolis: University of Minneapolis Press.

## L

1872 ..... 'The re-organization of academic study', *Athenaeum*, Nov. 23, 665 - 6.

## LANKESTER, E.R.

1870 ..... 'On the use of the term homology in modern zoology, and the distinction between homogenetic and homoplastic agreements', *Annals and magazine of natural history*, 4th series, 6, 34 - 43.

1873 ..... 'On the primitive cell-layers of the embryo as the basis of genealogical classification of animals, and on the origin of the vascular and lymphatic systems', *Annals and magazine of natural history*, 4th series, 11, 321 - 38.

1876 ..... 'An account of Professor Haeckel's recent additions to the gastraea theory', *QJMS*, new series, 16, 51 - 66.

1876 - 7 ..... 'The progress of embryology', *N*, 15, 65 - 6.

1877 ..... 'Notes on the embryology and classification of the animal kingdom: comprising a revision relative to the origin and significance of the germ layers', *QJMS*, new series, 17, 399 - 454.

1881 ..... 'Balfour's "Comparative embryology"', *N*, 25, 25 - 7.

1887 - 8 ..... 'Review of O. Hertwig, "Lehrbuch der Menschen und der Wirbelthiere"', N, 37, 506 - 8.

#### LARSON, J.L.

1979 ..... 'Vital forces: regulative principles or constitutive agents? A strategy in German physiology, 1786 - 1802', I, 70, 235 - 49.

#### LAWRENCE, W.

1822 ..... Lectures on physiology, zoology, and the natural history of man, J. Griffin.

#### LAYCOCK, T.

1833 - 57 ..... A journal, Edinburgh University Library MSS, Gen 1813.

1840 ..... A treatise on the nervous diseases of women: comprising an inquiry into the nature, causes, and treatment of spinal and hysterical disorders, Longmans.

[1844] ..... On the reflex function of the brain, C. and J. Adlard.

1846 ..... The medical police of the United Kingdom, Reynell and Wight.

1855 ..... 'Further researches into the functions of the brain', British and foreign chiurgical review, 16, 155 - 87.

1859 ..... 'Phrenology', Encyclopaedia Britannica, 8th ed., 17, 556 - 68.

1860 ..... Mind and brain: or the correlates of consciousness with organisation: with their applications to philosophy, zoology, physiology, mental pathology, and the practice of medicine, 2 vols, Edinburgh: Sutherland and Knox.

1862 ..... On the principles and method of a practical science of mind. A reply to criticism, J.E. Abelard.

1862a ..... The antagonism of law and medicine in insanity, and its consequences, Edinburgh: Oliver and Boyd.

#### LAYTON, D.

1973 ..... Science for the people: the origins of the school science curriculum in England, George Allen and Unwin.

#### LENIN, V.I.

1948 ..... Materialism and empirio-criticism, Moscow: Progress Publishers.

#### LEVERE, T.

1971 ..... Affinity and matter: elements of chemical philosophy 1800 - 1865, Oxford: Clarendon Press.

#### LEWES, G.H.

1875 ..... Problems of life and mind: series one, 2 vols, Trubner and co.

1879 ..... ditto: series two, 2 vols, Trubner and co.

1876 ..... 'Spiritualism and materialism', FR, old series, 19, 479 - 93; 707 - 19.

1877 ..... The physical basis of mind, Trubner and co.

#### LIDDELL, E.G.T.

1960 ..... The discovery of reflexes, Oxford: Clarendon Press.

#### LILLY, W.S.

1889 ..... A century of revolution, Chapman and Hall.

1890 ..... On right and wrong, Chapman and Hall.

#### LOEB, J.

1964 ..... The mechanistic conception of life (1912), new ed., Cambridge Mass: Belknap Press.

#### LONSDALE, H.

1870 ..... A sketch of the life and writings of Robert Knox the anatomist, Macmillan.

#### LUBBOCK, J.

1874 ..... On the metamorphosis of insects, Macmillan.

#### LYELL, C.

1845 ..... Travels in North America: with geological observations on the United States, Canada, and Nova Scotia, 2 vols, John Murray.

1881 ..... Life, letters and journals of Sir Charles Lyell, Bart., 2 vols, John Murray.

#### MCEVOY, J.G.

1978 ..... 'Joseph Priestley, 'Aerial philosopher': metaphysics and methodology in Priestley's chemical thought from 1762 to 1781', *Ambix*, 25, 1 - 45.

#### MCEVOY, J.G. and J.E. MCGUIRE

1975 ..... 'God and nature: Priestley's way of rational dissent', *Historical studies in the physical sciences*, 6, 325 - 404.

#### MACKENZIE, D.A.

1976 ..... 'Eugenics in Britain', *Social studies of science*, 6, 499 - 532.

1979 ..... 'Karl Pearson and the professional middle class', **AS**, **36**, 125 - 43.

1980 ..... **Statistics in Britain, 1865 - 1930: the social construction of scientific knowledge**, Edinburgh: Edinburgh University Press.

#### **MACLEOD, K.**

1908 ..... **Laycock**, Glasgow: Alex. Macdougall.

#### **MACLEOD, R.M.**

1965 ..... 'Evolutionism and Richard Owen, 1830 - 1868: an episode in Darwin's century', **I**, **56**, 259 - 80.

1971 ..... 'The support of Victorian science: the endowment of research movement in Great Britain 1868 - 1900', **Minerva**, **4**, 197 - 230.

#### **MCMULLIN, E.**

1978 ..... **Newton on matter and affinity**, Notre Dame: University of Notre Dame Press.

#### **MACH, E.**

1960 ..... **The science of mechanics: a critical and historical account of its development (1888)**, new ed., La Salle: Open Court Publishing Co.

#### **MACOBBY, S.**

1938 ..... **English radicalism 1853 - 1886**, Allen and Unwin.

1953 ..... **English radicalism 1886 - 1914**, Allen and Unwin.

#### **MAITLAND, F.W.**

1906 ..... **The life and letters of Leslie Stephen**, Duckworth and Co.

#### **MALLOCK, W.H.**

1881 ..... 'A missing science', **CR**, **40**, 934 - 58.

1883 ..... 'Socialism in England', **QR**, **156**, 353 - 93.

1884 ..... **Atheism and the value of life: five studies in contemporary literature**, Richard Bentley and Son.

1884a ..... **Property and progress or a brief inquiry into contemporary social agitation in England**, John Murray.

1889 ..... 'Mr John Morley and progressive radicalism', **QR**, **168**, 249 - 80.

1895 ..... 'Physics and sociology (part one)', **CR**, **68**, 883 - 908.

1896 ..... ditto (part two), **CR**, 69, 233 - 56.

**MANIER, E.**

1978 ..... *The young Darwin and his cultural circle*, Dordrecht: Reidel.

**MANNHEIM, K.**

1953 ..... *Essays on sociology and social psychology*, Routledge.

1962 ..... *Essays on the sociology of culture*, Routledge.

**MANNING, H.E.**

1883 ..... 'Without God, no commonwealth', **CR**, 44, 19 - 31.

**MANSEL, H.L.**

1858 ..... *The limits of religious thought*, Oxford: John Murray.

**MAPOTHER, E.D.**

1868 ..... *The medical profession: and its educational and licensing bodies*, Dublin: Finnin and Co.

**MARSH, P.T.**

1969 ..... *The Victorian Church in decline*, Routledge.

**MARSHALL, A.M.**

1893 ..... *Vertebrate embryology: a text-book for students and practitioners*, Smith, Elder and Co.

1897 ..... *Biological addresses and lectures*, 2nd ed., David Nutt.

**MARTINEAU, J.**

1860 ..... 'Cerebral psychology: Bain', *National review*, 10, 500 - 21.

1868 ..... *A word for scientific theology in appeal from the men of science and the theologians*, Williams and Norgate.

1886 ..... 'Expansion of the Church of England', **CR**, 50, 6 - 26.

1891 ..... *Essays, reviews, and addresses*, 4 vols, Longmans.

**MAUDSLEY, H.**

- 1867 ..... *The physiology and pathology of mind*, Macmillan.
- 1873 ..... *Responsibility in mental disease*, Bradbury, Agnew and Co.
- 1879 ..... *The pathology of mind*, Macmillan.
- 1883 ..... *Body and will: being an essay concerned with will in its metaphysical, physiological and pathological relations*, K. Paul, Trench and Co.

**MAULITZ, R.C.**

- 1978 ..... 'Rudolf Virchow, Julius Cohnheim, and the program of pathology', *BHM*, 52, 162 - 82.

**MAYO, T.**

- 1854 ..... *Medical testimony and evidence in cases of lunacy*, John Parker.

**MEACHAM, S.**

- 1972 ..... 'The sense of an impending clash: English working-class unrest before World War One', *American historical review*, 77, 1343 - 64.

**MILL, J.S.**

- 1965 ..... *Principles of political economy: with some of their applications to social philosophy*, new ed., 2 vols, Toronto: Toronto University Press.
- 1969 ..... *Essays on ethics, religion and society*, new ed., Toronto: Toronto University Press.
- 1971 ..... *Autobiography*, new ed., Oxford: Oxford University Press.
- 1972 ..... *The later letters of John Stuart Mill* (ed. F.E. Mineka and D.N. Lindley), Toronto: Toronto University Press.
- 1973 ..... *A system of logic, ratiocinative and inductive, being a connected view of the principles of evidence, and the methods of scientific investigation*, new ed., 2 vols, Toronto: Toronto University Press.
- 1977 ..... *Essays on politics and society*, new ed., Toronto: Toronto University Press.
- 1979 ..... *An examination of Sir William Hamilton's philosophy and of the principal questions discussed in his writings*, new ed., Toronto: Toronto University Press.

**MIVART, ST G.**

- 1885 ..... 'Taine on Jacobinism', *QR*, 161, 411 - 40.
- 1895 ..... 'Spencer versus Balfour', *NC*, 38, 261 - 77.

**MONRO, H.**

- 1851 ..... *Remarks on insanity: its nature and treatment*, John Churchill.



MOORE, G.E.

1903 ..... *Principia ethica*, Cambridge: Cambridge University Press.

MORGAN, C.L.

1890 - 1 ..... *Animal life and intelligence*, Edward Arnold.

MORGAN, T.C.

1819 ..... *Sketches of the philosophy of life*, Henry Colborn.

MORGAN, T.H.

1898 ..... 'Developmental mechanics', *Science*, new series, 7, 156 - 8.

MORLEY, J.

1874 ..... *The struggle for national education*, 3rd ed., Chapman and Hall.

1879 ..... *Diderot and the Encyclopaedists*, 2 vols, Macmillan.

1917 ..... *Recollections*, 2 vols, Macmillan.

MORRELL, J.B.

1971 ..... 'Individualism and the structure of British science in 1830', *Historical studies in the physical sciences*, 3, 183 - 204.

1973 ..... 'The patronage of mid-Victorian science in the University of Edinburgh', *Science Studies*, 3, 353 - 88.

MORRIS, B.R.

1844 ..... *A theory as to the proximate causes of insanity together with some observations upon the remote causes of the disease*, Henry Renshaw.

MÜLLER, F.

1869 ..... *Facts and arguments for Darwin*, John Murray.

MÜLLER, J.

9-42

1838 ..... *Elements of physiology* (trans. W. Baly), 2 vols, Taylor and Walton.

NEEDHAM, J.

1931 ..... *Chemical embryology*, 3 vols, Cambridge: Cambridge University Press.

[Newman, F.]

1855 ..... 'Theism', *Westminster review*, 64, 319 - 53.

OLSON, R.

1975 ..... *Scottish philosophy and British physics 1750 - 1880. A study in the foundations of the Victorian scientific style*, Princeton: Princeton University Press.

OPPENHEIMER, J.M.

1967 ..... *Essays in the history of embryology and biology*, Cambridge Mass: MIT Press.

OSLER, M.J.

1979 ..... 'Descartes and Charlton on nature and God', *JHI*, 40, 445 - 56 .

OSPOVAT, D.

1976 ..... 'The influence of Karl Ernst von Baer's embryology, 1828 - 1859: a reappraisal in the light of Richard Owen's and William Carpenter's "Palaentological application of Von Baer's law" ', *JHB*, 9, 1 - 28.

1978 ..... 'Perfect adaptation and teleological explanation: approaches to the problem of the history of life in the mid- nineteenth century', *Studies in the history of biology*, 2, 33 - 56.

OTTO, R.

1907 ..... *Naturalism and religion* (trans, J. A. and M. R. Thomson), Williams and Norgate.

OWEN, R.

1843 ..... *Lectures on the comparative anatomy and physiology of the invertebrate animals*, Longmans.

1855 ..... ditto, 2nd ed.,

1846 ..... 'Report on the archetype and homologies of the vertebrate skeleton', *Report of the British Association for the Advancement of Science (Southampton)*, 169 - 340.

1848 ..... *On the archetype and homologies of the vertebrate skeleton*, John van Voorst.

1849 ..... *On the nature of the limbs*, John van Voorst.

1859 ..... *On the classification and geographical distribution of the mammals*, John Parker.

1864 ..... *Instances of the power of God as manifested in his animal creation*, James Nisbet.

1866 - 8 ..... *On the anatomy of the vertebrates*, 3 vols, Longmans.

1883 ..... 'Embryological testimony to general homology', *Proceedings of the Zoological Society*, 51, 349 - 59.

**OWEN, R. (jr.)**

1894 ..... **The life of Richard Owen**, 2 vols, John Murray.

**PALEY, W.**

1836 ..... **Natural theology**, New ed., 2 vols, Charles Knight.

**PARKER, W.K.**

1869 ..... 'On the structure and development of the skull of the common fowl (*Gallus Domesticus*)', **Philosophical transactions of the Royal Society**, 159, 755 - 804.

1871 ..... 'On the structure and development of the skull of the common frog ( *Rana Temporaria*, L.)', **Philosophical transactions of the Royal Society**, 161, 137 - 203.

**PARKER, W.K. and G.T. BETTANY**

1877 ..... **The morphology of the skull**, Macmillan.

**PARSONS, H.G.**

1900 ..... 'The Tory future', **Blackwoods magazine**, 167, 182 - 93.

**PATTISON, M.**

1868 ..... **Suggestions on academical organisation with special reference to Oxford**, Edinburgh: Edmonston and Douglas.

**PEARSON, K.**

1892 ..... **The grammar of science**, Walter Scott.

1897 ..... **The chances of death and other studies in evolution**, 2 vols, Edward Arnold.

**PEEL, J.D.Y.**

1971 ..... **Herbert Spencer: the evolution of a sociologist**, New York: Basic Books.

**PERKIN, H.**

1978 ..... **The origins of modern English society 1780 - 1880**, 4th ed., Routledge.

**PERRY, R.B.**

1912 ..... **Present philosophical tendencies**, Longmans.

**PETERSON, M.J.**

1978 ..... *The medical profession in mid-Victorian London*, Berkeley: University of California Press.

**PICKSTONE, J.V.**

1977 ..... 'Absorption and osmosis: French physiology and physics in the early nineteenth century', *Physiologist*, 20, 30 - 7.

forthcoming ..... 'Bureaucracy, liberalism and the body in post-revolutionary France. Part 1: Bichat's physiology and the Paris school of medicine', *HS*.

**POCOCK, J.G.A.**

1972 ..... *Politics, language and time: essays on political thought and history*, 2nd ed., Methuen.

**P[OULTON], E.B.**

1886 - 7 ..... 'Review of A.C. Haddon, "An introduction to the study of embryology"', *N*, 36, 601 - 2.

**POYNTER, F.N.L.**

1968 ..... *Medicine and science in the 1860s*, Wellcome Institute.

**PRICHARD, J.C.**

1829 ..... *A review of the doctrine of a vital principle, as maintained by some writers on physiology*, John and Arthur Arch.

**PRINGLE-PATTISON, A. SETH**

1885 ..... *Scottish philosophy. A comparison of the Scottish and German answers to Hume*, William Blackwood.

1893 ..... 'The "new" psychology and automatism', *CR*, 63, 555 - 74.

1897 ..... *Man's place in the cosmos*, Edinburgh: William Blackwood.

1917 ..... *The idea of God in the light of recent philosophy*, Oxford: Clarendon Press.

**PROUT, W.**

1834 ..... *Chemistry, meteorology and the function of digestion considered with reference to natural theology*, William Pickering.

**RATHER, L.J.**

1961 ..... 'G.E. Stahl's physiological psychology', *BHM*, 35, 37 - 49.

**READER, W.J.**

1966 ..... **Professional men: the rise of the professional classes in nineteenth century England**, Weidenfeld and Nicolson.

**REDWOOD, J.**

1976 ..... **Reason, ridicule and religion: the age of Enlightenment in England 1666 - 1750**, Thames and Hudson.

**REHBOCK, P.F.**

1975 ..... 'Huxley, Haeckel, and the oceanographers: the case of *Bathyporus haeckelii*', *I*, 66, 504 - 33.

**RENNELL, T.**

1819 ..... **Remarks on scepticism, especially as it is connected with the subjects of organization and life: being an answer to the views of M. Bichat, Sir T.C. Morgan and Mr Lawrence, upon these points**, 2nd ed., F.C. and J. Rivington.

**RICHARDS, R.J.**

1977 ..... 'Lloyd Morgan's theory of instinct: from Darwinism to neo-Darwinism', *Journal of the history of the behavioural sciences*, 13, 12 - 32.

**ROACH, J.**

1957 ..... 'Liberalism and the Victorian intelligentsia', *Cambridge historical journal*, 13, 58 - 81.

**ROBERTS, D.**

1960 ..... **Victorian origins of the British welfare state**, New Haven: Yale University Press.

**RODERICK, G. and M. STEPHENS**

1974 ..... 'Scientific studies and scientific man-power in the English civic universities 1870 - 1914', *Science studies*, 4, 41 - 63.

**ROE, S.A.**

1979 ..... 'Rationalism and embryology: Caspar Friedrich Wolff's theory of epigenesis', *JHB*, 12, 1 - 43.

**ROMANES, G.N.**

1883 ..... **Mental evolution in animals**, Kegan Paul.

1888 ..... **Mental evolution in man: origin of human faculty**, Kegan Paul.

**ROSCOE, H.E.**

1869 - 70 ..... 'Science and education in Germany', **N, 1**, 157 - 8.

**ROTHBLATT, S.**

1968 ..... **The revolution of the dons: Cambridge and society in Victorian England**, Faber and Faber.

1976 ..... **Tradition and change in English liberal education: an essay in history and culture**, Faber and Faber.

**ROYLE, E.**

1973 ..... **Victorian infidels: the origins of the British secularist movement, 1791 - 1886**, Manchester: Manchester University Press.

**RUSSELL, E.S.**

1916 ..... **Form and function**, John Murray.

**SANDERSON, M. (ed.)**

1975 ..... **The universities in the nineteenth century**, Routledge.

**SCHAFFNER, K.F.**

1972 ..... **Nineteenth-century aether theories**, Oxford: Pergamon Press.

**SCHNEEWIND, E.H.**

1977 ..... **Sidgwick's ethics and Victorian moral philosophy**, Oxford: Clarendon Press.

**SCHOFIELD, R.E.**

1970 ..... **Mechanism and materialism. British natural philosophy in an age of reason**, Princeton: Princeton University Press.

**SCULL, A.**

1975 ..... 'From madness to mental illness: medical men as moral entrepreneurs', **Archives Européennes de sociologie**, **16**, 218 - 51.

1979 ..... **Museums of madness: the social organization of insanity in nineteenth century England**, Allen Lane.

**SEARLE, G.R.**

1971 ..... *The quest for national efficiency*, Oxford: Basil Blackwell.

1976 ..... *Eugenics in Britain, 1900 - 1914*, Leyden: Noordhoff International Publishing.

1978 ..... 'Eugenics and class', unpublished.

**SEDGWICK, A.**

1894 ..... 'On the law of development commonly known as von Baer's law; and on the significance of ancestral rudiments in embryonic development', *QJMS*, **36**, 35 - 52.

**SERRES, E.R.A.**

1824 ..... *Anatomie comparée du cerveau, dans les quatre classes des animaux vertébrés, appliqué à la physiologie et à la pathologie du système nerveux*, 2 vols, Paris: Chez Gabon.

**SHANNON, R.**

1976 ..... *The crisis of imperialism 1865 - 1915*, 2nd ed., St Albans: Paladin.

**SHAPIN, S.**

1975 ..... 'Phrenological knowledge and the social structure of early nineteenth-century Edinburgh', *AS*, **32**, 219 - 43.

forthcoming ..... *The social uses of science*, Edinburgh: Edinburgh University Press.

**SIDGWICK, H.**

1893 ..... *The methods of ethics*, 5th ed., Macmillan.

**SIMON, B. (ed.)**

1972 ..... *The radical tradition in education in Britain*, Lawrence and Wishart.

**SKINNER, Q.**

1969 ..... 'Understanding and explanation in the history of ideas', *History and theory*, **8**, 3 - 53.

**SMITH, CROSBIE**

1976 ..... ' "Mechanical philosophy" and the emergence of physics in Britain: 1800 - 1850', *AS*, **33**, 3 - 29.

**SMITH, C.U.M.**

1978 ..... 'Charles Darwin, the origin of consciousness, and panpsychism', *JHB*, 11, 245 - 67.

**SMITH, R.**

1970 ..... *Physiological psychology and the philosophy of nature in mid-nineteenth century Britain*, Cambridge PhD.

1972 ..... 'Alfred Russel Wallace: philosophy of nature and man', *BJHS*, 6, 177 - 99.

unpublished ..... *Trial by medicine: a social history of scientific knowledge and the insanity defence in Victorian trials*.

**SOFFER, R.N.**

1978 ..... *Ethics and society in England: the revolution in the social sciences 1870 - 1914*, Berkeley: University of California Press.

**SOLLY, S.**

1836 ..... *The human brain, its configuration, structure, development, and physiology: illustrated by reference to the nervous system of the lower orders of animal*, Longmans.

1847 ..... *The human brain: its structure, physiology and diseases. With a description of the typical forms of brain in the animal kingdom*, 2nd ed., Longmans.

**SPENCER, H.**

1855 ..... *The principles of psychology*, Longmans.

1870 ..... ditto, 2nd ed., 2 vols, Williams and Norgate.

1895 ..... 'Professional institutions. VI Men of Science', *CR*, 68, 538 - 47.

1896 ..... 'Professional institutions (concluded)', *CR*, 69, 547 - 53.

**STEPHEN, L.**

1962 ..... *History of English thought in the eighteenth century (1876)*, new ed., 2 vols, Rupert Hall.

**STEWART. B.**

1884 ..... 'The visible universe — is it a physical or a spiritual production?', *CR*, 46, 49 - 61.

**STEWART, B. and P.G. TAIT**

1894 ..... *The unseen universe: or physical speculations on a future state (1875)*, 17th ed., Macmillan.



**STIRLING, J.H.**

1869 ..... *As regards protoplasm: in relation to Professor Huxley's essay on the physical basis of life*, William Blackwood.

**STONE, L.**

1975 ..... *The university in society*, 2 vols, Princeton: Princeton University Press.

**STOUT, G.F.**

1891 ..... 'The scope and method of psychology', *Proceedings of the Aristotelian Society*, 1, 33 - 54.

1931 ..... *Mind and matter*, Cambridge: Cambridge University Press.

**STRICKER, S.**

1870 ..... 'The medical schools of England and Germany', *N*, 2, 349 - 50; 369.

**TAMMY, M.**

1979 ..... 'Newton, creation, and perception', *I*, 70, 48 - 58.

**TAX, S.**

1960 ..... *Evolution after Darwin*, 3 vols, Chicago: Chicago University Press.

**TEMKIN, O.**

1977 ..... *The double face of Janus and other essays in the history of medicine*, Baltimore: John Hopkins University Press.

**THACKRAY, A.**

1970 ..... *Atoms and powers: an essay on Newtonian matter-theory and the development of chemistry*, Cambridge Mass: Harvard University Press.

1972 ..... *John Dalton: critical assessments of his life and science*, Cambridge Mass: Harvard University Press.

**THACKRAY, A. and E. MENDELSON (eds.)**

1974 ..... *Science and values*, New York: Humanities Press.

**THOMPSON, D'ARCY W.**

1917 ..... *On growth and form*, Cambridge: Cambridge University Press.

**THOMPSON, E.P.**

1978 ..... *The making of the English working class*, 9th ed., Harmondsworth: Penguin.

**THOMSON, J.B.**

1870 ..... 'The hereditary nature of crime', *JMS*, 15, 487 - 98.

**TODD, R.B.**

1836 - 59 ..... *The cyclopaedia of anatomy and physiology*, 5 vols, Sherwood, Gilbert and Piper.

1845 ..... *The descriptive and physiological anatomy of the brain, spinal cord and ganglions, and of their coverings*, Sherwood, Gilbert and Piper.

**TODD, R.B and W. BOWMAN**

1845 ..... *The anatomy and physiology of man*, 2 vols, John Parker.

**TODDHUNTER, I.**

1876 ..... *William Whewell: an account of his writings*, 2 vols, Macmillan.

**T[UKE], D.H.**

1865 ..... 'Review of "The legal doctrine of responsibility in relation to the insane" by S.W. North', *JMS*, 11, 261 - 2.

**TULLOCH, J.**

1855 ..... *Theism: the witness of reason and nature to an all-wise and beneficent creator*, Edinburgh: William Blackwood.

**TURNER, F.M.**

1974 ..... 'Rainfall, plagues and the Prince of Wales: a chapter in the conflict of science and religion', *Journal of British studies*, 13, 46 - 65.

1974a ..... *Between science and religion: the reaction to scientific naturalism in late-Victorian Britain*, New Haven: Yale University Press.

1978 ..... 'The Victorian conflict between science and religion: a professional dimension', *I*, 69, 356 - 76.

**TYNDALL, J.**

1863 ..... *Heat considered as a mode of motion*, Longmans.

1878 ..... 'Consciousness, organisation, free-will, heredity, responsibility, etc', *JMS*, 24, 107 - 16.

1879 ..... **Fragments of science**, 2 vols, Longmans.

**UNZER, J.A. and G. PROCHASKA**

1851 ..... **"The principles of physiology" and "A dissertation on the functions of the nervous system"** (trans. T. Laycock), Sydenham Society.

**USCHMANN, G.**

1979 ..... 'Haeckel's biological materialism', **History and philosophy of the life sciences**, 1, 101 - 18.

**VIRCHOW, R.**

1860 ..... **Cellular pathology as based upon physiology and pathological histology** (trans. F. Chance), John Churchill.

**VON BAER, K.E.**

1853 ..... 'Fragments relating to philosophical zoology. Selected from the work of K.E. Von Baer' (trans, T.H. Huxley), **Scientific memoirs: natural history**, 1, 176 - 258.

**WACE, H.**

1878 ..... 'Scientific lectures — their use and abuse', **QR**, 145, 35 - 61.

**WARD, J.**

1899 ..... **Naturalism and agnosticism**, 2 vols, Adam and Charles Black.

1927 ..... **Essays in philosophy**, Cambridge: Cambridge University Press.

**WATTS, R.**

1888 ..... **The reign of causality: a vindication of the scientific principle of telic causal efficiency**, Edinburgh: T. and T. Clark.

**WEISMANN, A.**

1882 ..... **Studies in the theory of descent** (trans. R. Meldola), Sampson, Low, Marston.

**WHEWELL, W.**

1833 ..... **Astronomy and general physics considered with reference to natural theology**, William Pickering.

1842 ..... **The philosophy of the inductive sciences, founded upon their history**, 2 vols, 2nd ed., John Parker.

1845 ..... **The elements of morality, including polity**, 2 vols, John Parker.

1846 ..... **Lectures on systematic morality**, John Parker.

**WIGHTMAN, W.P.D.**

1934 ..... **Science and monism**, Allen and Unwin.

**WILDE, C.**

1980 ..... 'Hutchinsonianism, natural philosophy and religious controversy in eighteenth century Britain', **HS**, 18, 1 - 24.

**WILLIS, R. (ed.)**

1979 ..... **On the margins of science: the social construction of rejected knowledge**, Keele: Sociological Review Monographs.

**WILSON, D.B.**

1974 ..... 'Herschel and Whewell's version of Newtonianism', **JHI**, 35, 79 - 97.

1974a ..... 'Kelvin's scientific realism: the theological context', **Philosophical journal**, 11, 41 - 60.

**WILSON, L.G.**

1972 ..... **Charles Lyell the years to 1841: the revolution in geology**, New Haven: Yale University Press.

**WINSLOW, F.**

1860 ..... **On obscure diseases of the brain, and disorders of the mind: their incipient symptoms, pathology, diagnosis, treatment, and prophylaxis**, John Churchill.

**WINSOR, M.P.**

1976 ..... **Starfish, jellyfish, and the order of life. Issues in nineteenth-century science**, New Haven: Yale University Press.

**WINSTANLEY, D.A.**

1947 ..... **Later Victorian Cambridge**, Cambridge: Cambridge University Press.

**WYNNE, B.**

1976 ..... **C.G. Barkla and the J phenomenon. A case study in the sociology of physics**, Edinburgh M.Phil.

**YATES, F.A.**

1969 ..... *Giordano Bruno and the Hermetic tradition*, New York: Vintage Books.

**YEO, R.**

1977 ..... *Natural theology and the philosophy of knowledge in Britain, 1819 - 1869*, Sydney PhD.

**YOUNG, G.M.**

1937 ..... *Victorian England: portrait of an age*, 2nd ed., Oxford University Press.

**YOUNG, J.**

1869 ..... 'Professor Huxley's "The physical basis of life"', *CR*, 11, 240 - 63.

**YOUNG, K.**

1963 ..... *Arthur James Balfour. The happy life of the politician, Prime Minister, statesman and philosopher*, G. Bell and Sons.

**YOUNG, R.M.**

1969 ..... 'Malthus and the evolutionists: the common context of biological and social theory, Past and present, 4, 109 - 45.

1970 ..... *Mind, brain and adaptation in the nineteenth century. Cerebral localization and its biological context from Gall to Ferrier*, Oxford: Clarendon Press.

**ZEBEL, S.H.**

1973 ..... *Balfour: a political biography*, Cambridge: Cambridge University Press.

## REFERENCES

## Introduction

1. Martineau, 1891, vol 2, 1.
2. Goldmann, 1964, 7.
3. See A.J. Balfour, 1895; A. Seth Pringle-Pattison, 'The use of the term "naturalism"', (1896), in idem, 1897, 289 - 303; T.H. Huxley, 1895.
4. Moore, 1903, 37 - 8.
5. Ibid, 40.
6. Ibid.
7. Ward, 1899, vol 1, 5.
8. Ibid, 186.
9. Ibid, 14.
10. Cf. Brush, 1967.
11. Ward, 1899, vol 1, 19 - 20.
12. Otto, 1907, 18 - 20.
13. Ibid, 37.
14. Wightman, 1934.
15. Stephen, 1962, vol 1, 26.
16. Ibid, 26 - 7.
17. Ward, 1899, vol 1, 19 - 20;
18. Perry, 1912, 63.
19. See Skinner, 1969; Pocock, 1972.
20. Skinner, 1969, 40.
21. See Barnes, 1977, chap. 3.
22. Mannheim, 1962, 53.
23. Goldmann, 1964; MacKenzie, 1979, 125 - 8.
24. Giddens, 1977, 85 - 6.
25. Ibid, 93.
26. See Shapin, forthcoming, chap. 6.

## Chapter One: The Context of Naturalism

1. Morrell, 1973, 353 - 4.
2. Cf. E. Mendelsohn, 'The emergence of science as a profession in nineteenth-century Europe', in Hill, 1964, 3 - 48.
3. Booth, 1886, 365, 385.
4. Thackray and Mendelsohn, 1974, 9; Turner, 1978, 361 - 2.
5. Roderick and Stephens, 1974, 41 - 59; Kargon, 1977.
6. Thackray, 1972, 10.
7. Kuhn, 1970.
8. Crossley, 1979, 7 - 8.
9. Thackray, 1972, 11.
10. T.H. Huxley, 1893 - 4, vol 2, 52.
11. Ward, 1899, vol 1, 5.
12. T.H. Huxley, 1893 - 4, vol 1, 159.
13. Laycock, 1860, vol 1, 75.
14. Cf. Brooke, 1977 and idem, 'The natural theology of the geologists : some theological strata', in Jordanova and Porter, 1979, 39 - 64; Gillispie, 1959; Yeo, 1977, chap. 1, 39 - 64.
15. See Brock, 1966.
16. See T.H. Huxley, 1893 - 4, vol 5, VII - VIII, 20 - 3.
17. Turner, 1978, 361.
18. L. Huxley, 1900, vol 1, 178.
19. Cf. Gruber and Barrett, 1974, 19 - 72; Manier, 1978, 64 - 8.
20. L.G. Wilson, 1972, 309 - 10.
21. Lyell, 1881, vol 2, 279 - 80.
22. Brooke, 1977a.
23. Lyell to Huxley, 20.2.1864, **Huxley Papers**, 6. 97; **Record**, 15.2.64.
24. L. Huxley, 1900, vol 1, 178.
25. Ibid, 202 - 3.
26. Hobsbawm, 1962, 151 - 4, 243 - 52; Brock and Macleod, 1976.
27. L. Huxley, 1900, vol 1, 397.
28. T.H. Huxley, 1894, 2.
29. Anon, 1864, 123.
30. Ibid.
31. Tyndall to Huxley, 22.4.1869., **Huxley Papers**, 8.70.
32. Abbot and Campbell, vol 1, 290 - 3.
33. L. Huxley, 1900, vol 1, 221.
34. Morrell, 1971, esp. 191 - 200; Cardwell, 1972.
35. See Appendix A.
36. Quoted in Basalla et al., 1970, 9.
37. L. Huxley, 1900, vol 1, 66.
38. Ibid, 67 - 9.
39. Ibid.
40. Commission, 1874, vol 2, 120.
41. Eve and Creasey, 1945, 31 - 6.
42. Ibid, 41 - 3.
43. Babbage, 1851, 171.
44. Ibid, 174 - 5.
45. Anon, 1865, 61.
46. Ibid, 62.
47. Brodie, 1872 - 3, 97.
48. Crossley, 1979, 9 - 10.
49. Thackray, 1974, 15.
50. Commission, 1874, vol 2, 75.
51. Ibid, 89 - 90.

52. See Roberts, 1960.
53. Commission, 1874, vol 2, 121.
54. T.H. Huxley, 1893 - 4, vol 3, 87.
55. Commission, 1864, 370.
56. Crossley, 1979, 76; Rothblatt, 1976.
57. Winstanley, 1947, 185 - 7.
58. Brodie, 1872 - 3., 97.
59. Roscoe, 1869 - 70, 157.
60. Commission, 1864, 370 - 2.
61. L. Huxley, 1900, vol 1, 277.
62. BAAS, 1867, xlv.
63. Commission, 1874a, 197 - 206.
64. Ibid, 202.
65. Ibid. These were not, however, the only funds available for chairs.
66. BAAS, 1867, xlv.
67. Ibid, xlv.
68. Babbage, 1851, 207.
69. Sanderson, 1975, 2 - 9.
70. Quoted in Adamson, 1930, 189.
71. Ibid.
72. Brodie, 1872 - 3, 98.
73. Marsh, 1969, 67.
74. Layton, 1973, 117.
75. Ibid, 152 - 4.
76. Lyell, 1881, vol 2, 86 - 7.
77. Lyell, 1845, 295 - 6.
78. Darwin to Lyell, 2[?] .8.1845., **Darwin-Lyell Correspondence**, Bn 25L (Carroll #44).
79. Lyell, 1881, vol 2, 127.
80. Lyell to Huxley, 16.3.1860., **Huxley Papers**, 6.32.
81. Lyell to Huxley, 21.5. 1860., *ibid*, 6.34.
82. Perkin, 1978, 252 - 70, 428 - 9.
83. A. Engel, 'Emerging concepts of an academic profession at Oxford 1800 - 1854', in Stone, 1975, vol 1, 305 - 51 (307 - 15); Rothblatt, 1968.
84. Pattison, 1868, 324.
85. Ibid, 102, 325 - 7.
86. Lankester to Huxley, 18.12.1872., **Huxley Papers**, 21.39 - 40.
87. Macleod, 1971.
88. Brodie, 1872 - 3, 98.
89. L, 1872, 665.
90. Perkin, 1978. 255 - 6.
91. T.H. Huxley, 1869, 654.
92. Maitland, 1906, 175 - 7;
93. G.M. Young, 1937, 96.
94. Mill, 1971, 102 - 3.
95. Adamson, 1930, 128, 273 - 8.
96. Marsh, 1969, 71.
97. Macobby, 1938, 150 - 78.
98. Hirst, 1927, vol 2, 131. See also Everett, 1939, 106 - 44.
99. Morley, 1879, vol 1, 4.
100. Cooter, 1978, 212 - 3.
101. R. Carlile, 'An address to men of science', in Simon, 1972, 95 - 137 (100 - 1)
102. Haevey, 1934, 434 - 55.
103. Buckle, 1858 - 61, vol 1, 350 - 1, 355.
104. Ibid, 836.



105. Ibid, 822.
106. Ibid.
107. Morley, 1917, vol 1, 25.
108. Morley, 1879, vol 1, 172.
109. Ibid, vol 2, 185.
110. **Parliamentary debates**, 1871, 204, 500, 526, 503 - 4, 520.
111. Fawcett to Huxley 14.1.1878., **Huxley Papers**, 16.51 - 2.
112. Conway, 1904, vol 2, 352 - 5. See also Conway to Huxley, 13.11.1878., **Huxley Papers**, 12.298 and idem, 7.1.1879., ibid, 12.300; Stephen to Huxley, 19.11.1878., ibid, 27.50 and idem, 17.1.1879., ibid, 27.53; Romanes to Huxley, 3.1.79., ibid, 23. 206.
113. Huxley to Lyell, 17.3.1860., **Darwin-Lyell Correspondence**, BD25L; in reply to Lyell to Huxley, 16.3.1860., **Huxley Papers**, 6.32.
114. E. Darwin, 1915, vol 2, 169.
115. F. Darwin, 1887, vol 2, 110, 386, 315.
116. Darwin to Lyell, op cit (105).
117. Gruber and Barrett, 1974, 67 - 8.
118. **Pall Mall Gazette**, 2.8.1866 and 3.8.1866.
119. Farrar, 1863, 63.

## Chapter Two: Causes, and Forces, and Powers

1. Quoted in Clifford, 1879, vol 1, 109.
2. Campbell and Garnett, 1882, 639 - 40.
3. Heimann and McGuire, 1971, 236; see also McMullin, 1978, 43ff.
4. Heimann, 1978.
5. See Thackray, 1970, and Schofield, 1970.
6. Ibid.
7. Kuypers, 1930, esp. Chs 2 - 3.
8. Hume, 1817, vol 1, 246 - 50.
9. Ibid, 294.
10. Ibid, 296 - 8.
11. Ibid, 329 - 30.
12. Ibid, 230, 235.
13. Hume, 1927, 69 - 70.
14. Ibid, 71. Cf. Osler, 1979.
15. See Redwood, 1976, and Hurlbutt, 1965.
16. Davy, 1839 - 40. vol 1. 225.
17. E. Gellner, 'French materialism', idem, 1974, 113 - 48 (126).
18. C.C. Gillispie, 'The *Encyclopédie* and the Jacobin philosophy of science: a study in ideas and their consequences', in Clagett, 1962, 255 - 89.
19. Farrar, 1862, 274 - 5. See also Royle, 1973, Chap. One.
20. Garfinkle, 1955, 377.
21. McEvoy and McGuire, 1975; Garrett, 1973; McEvoy, 1978.
22. Quoted in J. Martineau, 'Personal influences on present theology' (1856), idem, 1891, vol 1, 219 - 81 (237).
23. Ibid.
24. Ibid, 253 - 8.
25. Crosbie Smith, 1976, 4 - 5. See also Olson, 1975, esp. 252 - 70.
26. See S.F. Cannon, 'Science as a norm of truth', idem, 1978, 1 - 28 (esp. 7 - 8); idem, 'The Cambridge network', ibid, 29 - 71.
27. D.B. Wilson, 1974, 79 - 80. John William Frederick Herschel (1792 - 1871) mathematician and astronomer; Master of the Mint after 1850. William Whewell (1794 - 1866) Master of Trinity College, Cambridge from 1841. Wrote on mechanics and mineralogy as well as on philosophy.
28. Whewell, 'Reflexions on God', Todhunter, 1876, vol 1, 360 - 5 (360 - 1);
29. Ibid, 362 - 3.
30. Ibid, 363 - 4.
31. Whewell, 1833, 5.
32. Ibid, 302.
33. Ibid, 361 - 2.
34. Whewell, 1842, vol 1, 66.
35. Ibid, 166 - 9.
36. Ibid, vol 2, 434 - 5.
37. Herschel, 1966, 7.
38. Ibid, 4.
39. Ibid, 144 - 9.
40. Ibid, 222 - 3.
41. Ibid, 86.
42. Ibid, 38.
43. Herschel, 1833, 232.
44. Ibid, 232 - 3.
45. J. Herschel, 'Mechanism of the heavens' (1833), idem, 1857, 21 - 62 (28).
46. J. Herschel, 'An address', idem, 1857, 634 - 82 (674).
47. Ibid, 677.
48. J. Herschel, 'On atoms' (1860), idem, 1866, 452 - 60 (458).

49. Cf. Levere, 1971, 24 - 30.
50. Ibid, 78.
51. Exley, 1829, vi, xix-xxi.
52. Ibid, xxvi - xxvii.
53. See Tammy, 1979.
54. Exley, 1829., xxvii.
55. Tulloch, 1855, 38.
56. Ibid, 322 - 3
57. Ibid, 324 - 5.
58. R.M. Young, 1969.
59. Levere, 1971, 79.
60. Kargon, 1977; Thackray, 1972.
61. Crossley, 1979, 241.
62. Ibid, 237.
63. Quoted in Brock and Macleod, 1976, 48.
64. Hooker, 1868, lxxii - lxxiii.
65. Ibid.
66. Newman, 1855, 323 - 4.
67. A. Barry, 1869, 236.
68. J. Martineau, 'Nature and God' (1860), idem, 1891, vol 3, 143 - 83 (148).
69. Martineau, 1868, 16 - 7;
70. A. Barry, 1869, 241.
71. Martineau, 1891, vol 3, 159.
72. Ibid, 154 - 5.
73. Martineau, 1868, 23 - 4.
74. J. Herschel, 'On the origin of force' (1865), idem, 1866, 460 - 75 (460 - 3).
75. Ibid, 464 - 5.
76. J. Martineau, 'Modern materialism its attitude towards theology' (1876), idem, 1891, vol 4, 197 - 267 (208 - 9; 234).
77. Anon, 1864a, 43 - 4.
78. Grove, 1855, 15 - 6. See also 21 - 37.
79. J. Tyndall, 'Introduction' to Helmholtz, 1873, xv - xvi.
80. Tyndall, 1863, x.
81. Ibid, 60.
82. J. Tyndall, 'The constitution of nature' (1865), idem, 1879, vol 1, 3 - 27 (3 - 11).
83. Ibid, 13 - 5.
84. Ibid, 16.
85. Ibid, 19.
86. Ibid.
87. This is a version of the first law of thermodynamics. Tyndall did not mention the apparent contradiction of this point by the **second** law.
88. J. Tyndall, 'Reflections on prayer and natural law' (1861), idem, 1879, vol 2, 1 - 7. See also Turner, 1974.
89. Tyndall, 1878, 107 - 8.
90. Tyndall, 1863, 433 - 4.
91. Yates, 1969, esp. 234 - 7, 242 - 6.
92. Turner, 1978, 372 - 4.
93. J. Tyndall, 'The Belfast Address', idem, 1879, vol 2, 137 - 203 (199).
94. Ibid, 137 - 9.
95. Ibid, 156.
96. Ibid, 161 - 2.
97. Eve and Creasey, 1945, 187 - 8.
98. Wace, 1878, 37 - 9.
99. Watts, 1888, 8.
100. Ibid, 14 - 5.

101. Ibid, 27, 19.
102. Davie, 1964, esp. Ch 8.
103. Heimann, 1972. . Balfour Stewart (1828 - 87) studied natural philosophy at Edinburgh under James Forbes. He specialised in research into radiant heat, and became Professor of Natural Philosophy at Owen's College, Manchester in 1870. He was a fervent Churchman and a founder member of the Society for Psychical Research.  
Peter Guthrie Tait (1831 - 1901) helped to restore Newtonian mechanics in nineteenth century Britain. He was acquainted with W.R. Hamilton and helped perfect the latter's theory of quaternions. Tait collaborated with Kelvin in expounding the doctrine of the conservation of energy.
104. Stewart and Tait, 1894, xi, xviii.
105. Ibid, 14.
106. Ibid, 100 - 15.
107. See Schaffner, 1972 and Doran, 1975.
108. Stewart and Tait, 1894, 155 - 6.
109. Ibid, 199.
110. Stewart, 1884, 48.
111. D.B. Wilson, 1974a; B. Wynne, 1976; idem, 'Physics and psychics', in Barnes and Shapin, 1979, 167 - 86.
112. Lenin, 1948, 371.
113. See Pringle-Pattison, 1885.
114. Cf. Hamburger, 1965.
115. Mill, 1972, 149.
116. Mill, 1973, vol 1, 326.
117. Ibid, 327.
118. Ibid, 340.
119. Ibid, 353 - 4.
120. Ibid, 355.
121. Bain, 1859, 472 - 3.
122. Ibid, 475 - 6.
123. Ibid, 477.
124. Lewes, 1875, vol 2, 361, 386.
125. Ibid, 402 - 3.
126. Ibid, 381.
127. Ibid, 309ff.
128. Ibid, 317 - 8.
129. Ibid, 355.
130. Quoted in Brush, 1976, vol 1, 286.
131. Helmholtz, 1853, 116 - 7.
132. Mach, 1960, 94, 307.
133. Ibid, 95.
134. Clifford, 1878 - 87, vol 1, 1.
135. Ibid, 2.
136. Ibid, 3.
137. Ibid, vol 2, 73.
138. K. Pearson, 'Preface' to Clifford, 1946, lxi - lxxv (lxxv).
139. Pearson, 1892, Chap.4.
140. K. Pearson, in Clifford, 1946, 242 - 3.
141. W.K. Clifford, 'On theories of the physical forces' (1870), idem, 1879, vol 1, 109 - 23 (109 - 10).
142. Ibid, 123.
143. W.K. Clifford, 'The philosophy of the pure sciences' (1873), idem, 1879, vol 1, 254 - 340 (261). See also J.L. Richards, 'The reception of a mathematical theory', in Barnes and Shapin, 1979, 143 - 60.
144. Ibid, 269 - 79.
145. W.K. Clifford, 'On the aims and instruments of scientific thought' (1872), idem, 1879, vol 1,

- 124 - 57 (126).
146. Ibid, 156 - 7.
  147. Clifford, 1877, 343.
  148. Ibid, 357 - 8.
  149. On Pearson see MacKenzie, 1979.
  150. K. Pearson, 'Politics and science' (1894), *idem*, 1897, vol 1, 140 - 72 (141ff).
  151. Ibid, 148.
  152. K. Pearson, 'Reaction' (1895), *idem*, 1897, 173 - 225 (201 - 4).
  153. Ibid, 224.
  154. Pearson, 1892, 7.
  155. Ibid, 9 - 10.
  156. Ibid, 30.
  157. Ibid, 4.
  158. Quoted in Redwood, 1976, 49.
  159. Hobbes, 1968, 397.
  160. Whewell, 1845, vol 1, 48.
  161. Ibid, vol 2, 2 - 3.
  162. Ibid, 7.
  163. Ibid, 32 - 3.
  164. Ibid, 49, 178.
  165. J. Martineau, 'Whewell's morality' (1846), *idem*, 1891, vol 3, 377 - 407 (377 - 8).
  166. Ibid, 378.
  167. Joseph Goodsir, 1868, 35, 45 - 7.
  168. Ibid, 59 - 61.
  169. Ibid, 62.
  170. Whewell, 1845, vol 2, 357.
  171. Whewell, 1846, 197.
  172. Drummond and Upton, 1902, vol 2, 268 - 9, James Martineau (1805 - 1900) was a Unitarian minister who served as Professor of Mental and Moral Philosophy at Manchester New College from 1840. He was an 'Old Whig' in politics who strongly supported the existence of a national church.
  173. Ibid, 278, 340, 271 - 2, 452 - 4.
  174. J. Martineau, 'Church and state' (1845), *idem*, 1891, vol 2, 1 - 45 (34 - 5).
  175. Ibid, 36.
  176. Ibid, 40 - 5.
  177. J. Martineau, 'John Stuart Mill' (1859), *idem*, 1891, vol 3, 489 - 536 (512).
  178. Ibid, 516 - 8.
  179. Argyll, 1867, 67, 118 - 24, 215.
  180. Ibid, 418.
  181. Ibid, 418 - 21.
  182. These points are discussed further in Chap. 6 below.
  183. J.S. Mill, 'Sedgwick's discourse' (1835), *idem*, 1969, 33 - 74 (53).
  184. J.S. Mill, 'Whewell on moral philosophy' (1852), *ibid*, 167 - 201 (167 - 8, 200).
  185. Cf. Anschutz, 1953, 6.
  186. Mill, 1971, 134 - 5.
  187. Mill, 1972, 723.
  188. Mill, 1971, 135.
  189. Mill to Bain, 7.1.1863, Mill, 1972, 817.
  190. Mansel, 1858, esp. 108 - 11.
  191. Mill, 1974, 89 - 101;
  192. Mill, 1977, 374 - 5.
  193. Mill, 1965, vol 2, 759.
  194. Ibid, 763 - 4.
  195. Mill, 'Three essays on religion' (1873), *idem*, 1969, 371 - 489 (377 - 83).

## Chapter Three: The Physical Basis of Life

1. Cf. Benton, 1974 for an attempt at classification.
2. Quoted in T.S. Hall, 1969, vol 1, 104.
3. Larson, 1979, 237.
4. Ibid.
5. On Stahl see T.S. Hall, 1969, vol 1, 357 - 62; Rather, 1961.
6. On Hunter see Goodfield, 1960, 108.
7. Lawrence, 1822, 63. William Lawrence (1783 - 1867) was a surgeon, anthropologist, alienist, and a former pupil of Abernethy. He was a member of the reform party in the medical profession and supported **The Lancet** faction against the medical establishment.
8. Ibid, 70.
9. Ibid, 67 - 73.
10. Ibid, 74.
11. Ibid, 7.
12. Cf. O. Temkin, 'Basic science, medicine, and the Romantic era', idem, 1977, 345 - 72 (367 - 70).
13. Lawrence, 1822, 10.
14. Temkin, 1977, 345 - 72; Goodfield, 1969.
15. Abernethy, 1815, 16 - 7. John Abernethy (1764 - 1831) was a pupil of Hunter and a practising surgeon as well as an anatomist and physiologist. He lectured at the Royal College of Surgeons from 1814 - 1817.
16. Ibid, 39.
17. Hood, 1822, 187 - 8.
18. Abernethy, 1817, 27 - 30.
19. Ibid, 46.
20. Ibid, 54, 60.
21. T.C. Morgan, 1819, 6 - 7. Thomas Charles Morgan (1783 - 1843) was a Cambridge MD and a Fellow of the Royal College of Physicians. He was a Francophile and politically liberal. Hostile reaction to his writings on life forced him to abandon his medical practice.
22. Ibid, 7.
23. Ibid, 74 - 6.
24. Ibid, 381 - 2.
25. Ibid, 381.
26. Rennell, 1819, 64, 53. Thomas Rennell (1787 - 1824) was a Highchurch cleric, and 'Christian Advocate' at Cambridge University after 1816. His attack upon Morgan and Lawrence went through six editions by 1824.
27. Abernethy, 1821, 72.
28. John Barclay (1758 - 1826) was a cleric who turned to science. He lectured on anatomy at Edinburgh from 1797 - 1825.
29. Barclay, 1822, vii.
30. Ibid.
31. Ibid, 141.
32. Ibid, 288.
33. Ibid, 297, 293 - 4.
34. Ibid, 77.
35. Quoted ibid, 302.
36. Ibid, 309.
37. Ibid, 52, 523.
38. E.P. Thompson, 1978, esp. 781 - 819.
39. Halévy, 1934.
40. R. Carlile, 'An address to men of science' (1822), in Simon, 1972, 95 - 137, (98).
41. Ibid, 101 - 3.
42. Ibid, 103.
43. Ibid, 110.
44. Rennell, 1819, 50 - 1.

45. Lawrence, 1822, 10.
46. C. Bell, 'Appendix' to Paley, 1836, vol 2, 211 - 424 (402 - 3).
47. Prout, 1834, 433 - 4, 441.
48. Kirby, 1852, 1. On the Hutchinsonians see Wilde, 1980.
49. Kirby, 1852, 7.
30. Ibid, 14.
51. Ibid, 50.
52. Ibid.
53. Ibid, 52.
54. On the earlier period see J.R. Jacob, 1977; M.C. Jacob, 1976.
55. Prichard, 1829, 72; see also vii. James Cowles Prichard (1786 - 1848) was an Edinburgh-trained physician who made his home in Bristol. He was chiefly known for his anthropological writings.
56. Ibid, 132.
57. Ibid, 141 - 2.
58. Ibid, 224.
59. Alison, 1830, iv.
60. Ibid, 7.
61. Cuvier, 1859, 4 - 5.
62. On Bichat see Coleman, 1977, 20 - 1; Pickstone, forthcoming.
63. Fletcher, 1835 - 6, part 1, xix; part 2a, 33.
64. Ibid, part 2a, 16-- 7, 37 - 8.
65. Lonsdale, 1870, 17 - 8.
66. Ibid, 121 - 2, 358.
67. John Goodsir (1814 - 67) studied anatomy at Edinburgh under Knox. In 1841 he became curator of the museum of the Royal College of Surgeons in Edinburgh where he lectured on minute anatomy. In 1845 he was appointed Professor of Anatomy at Edinburgh University.
68. John Goodsir, 1868, vol 1, 63 - 5.
69. Carpenter, 1837 - 8, 330.
70. Ibid, 331 - 3.
71. Ibid, 347.
72. Ibid, 349.
73. Ibid, 351.
74. W.B. Carpenter, 'Life', in Todd, 1836 - 59, vol 3, 141 - 60 (143 - 4).
75. Ibid, 142, 145 - 6.
76. Carpenter, 1839, 350.
77. Carpenter, 1842, 538.
78. Ibid, third edition, 1846, 631.
79. Ibid, first edition, 1842, 542.
80. Ibid, third edition, 1846, 636.
81. Carpenter, 1855, 359.
82. Carpenter, 1847, 39.
83. Carpenter, 1855a, 411 - 2.
84. Ibid, 412.
85. Carpenter, 1847, 34.
86. Ibid, second edition, 1859, 42 - 3, 229 - 30.
87. T.S. Hall, 1969, vol 2, 179 - 209; Baker, 1948 - 53; Coleman, 1965; Pickstone, 1977.
88. Prichard, 1829, 229 - 30.
89. R. Willis, 'Animal', in Todd, 1836 - 59, vol 1, 118 - 47 (120 - 1).
90. R.D. Grainger, 'Cellular tissue', *ibid*, 509 - 17 (511).
91. W.B. Carpenter, 'The microscope', *ibid*, vol 3, 331 - 58 (357 - 8).
92. T.R. Jones, 1855, 13.
93. Cf. M. Barry, 1837.
94. John Goodsir, 1868, vol 2, 379 - 86 (379 - 81).
95. John Goodsir, 1845, 1 - 2.
96. Carpenter, 1856, 28.

97. Ibid.
98. Cf. W. Bowman, 'On the minute structure and movements of the voluntary muscles' (1840 - 1), in *idem*, 1892, vol 1, 3 - 48.
99. Todd and Bowman, 1845, vol 1, 51.
100. Several of Goodsir's students' lecture notes survive: see especially, Edinburgh University Library MSS, Gen 1428.
101. Beale, 1856, 1 - 3.
102. Ibid, 39 - 41.
103. Ibid, 45 - 9.
104. Ibid, 65 - 71.
105. Beale, 1857, 1 - 2.
106. Ibid, 3 - 4.
107. Ibid, 85 - 7;
108. Ibid, 48 - 9;
109. Ibid, 97.
110. Ibid.
111. See Todd and Bowman, 1845, vol 1, 13.
112. T.H. Huxley, 'On the identity of structure of plants and animals' (1853), in *idem*, 1898, vol 1, 216 - 20 (216 - 7).
113. Ibid, 219.
114. Cf. Farley, 1977, 54.
115. John Goodsir, 1868, vol 1, 176 - 8, 180, 193 - 4; vol 2, 205 - 19.
116. Ibid, vol 2, 219.
117. L. Huxley, 1900, vol 1, 140.
118. Ibid.
119. T.H. Huxley, 'The cell theory' (1853), in *idem*, 1898, vol 1, 241 - 78 (278).
120. Ibid.
121. Ibid, 246.
122. Roe, 1979, 39.
123. T.H. Huxley, 1898, vol 1, 253 - 4.
124. *Idem*, 277.
125. Ibid, 278.
126. T.H. Huxley, 'The principles of biology' (1858), **Huxley Papers**, 32.5.
127. Ibid, 32.51 - 6.
128. Ibid, 32.59.
129. T.H. Huxley, 1877, 9.
130. Todd and Bowman, 1845, vol 1, 31.
131. John Burdon Sanderson (1828 - 1905) studied medicine in Edinburgh in the 1840s. He then went to Paris to study chemistry under Gerhardt and Wurtz and physiology under Bernard. He succeeded Foster as Professor of Practical Physiology at University College in 1871, and in 1882 was appointed Regius Professor of Physiology at Oxford.  
 Frederick William Pavy (1829 - 1911) was a London MB who went to Paris in 1853 to work under Bernard. He lectured at Guy's Hospital upon his return to Britain as well as doing original work in chemical physiology.  
 William Rutherford (1839 - 99) was an Edinburgh MD who studied under du Bois Reymond and Ludwig as well as in Paris. He became Professor of Physiology at King's College London in 1869; in that capacity he stressed the importance of physiological chemistry and experimental physiology.
132. French, 1971, 32. Edward Albert Sharpey-Schäffer (1850 - 1935) studied medicine at University College. He became Professor of Physiology there in 1883; and Professor of Practical Physiology at Edinburgh in 1899. His interests included the chemistry of the blood; he also did pioneering work in endocrinology and in 'physiological histology' — most notably, of the neurons.  
 Victor Horsley (1857 - 1916) was a medical graduate of University College. He worked under Burdon Sanderson at the Brown Institute 1884 - 1900. In 1885 he was appointed Professor of Pathology at University College.



Walter Holbrooke Gaskell (1847 - 1914) was one of Foster's earliest Cambridge discoveries. He worked with Ludwig in Leipzig 1874-5 then returned to Cambridge. His chief work was on the autonomic nervous system.

133. J. Burdon Sanderson, 'The origin and meaning of the term "biology"' (1893), in G. Burdon Sanderson, 1911, 235 - 70 (244).
134. J. Burdon Sanderson, 'Elementary problems in physiology' (1889), in G. Burdon Sanderson, 1911, 215 - 34 (217 - 8).
135. T.H. Huxley, 'On the educational value of the natural history sciences' (1854), in idem, 1893 - 4, vol 3, 38 - 65 (49 - 50).
136. Geison, 1972, 51.
137. Bastian, 1870, 425.
138. Gasman, 1971, 55 - 76; Uschmann, 1979.
139. Rehbock, 1975.
140. Coleman, 1965, 128.
141. Huxley, 1866, v.
142. Ibid, 3.
143. Ibid, 288 - 9.
144. Quoted in Geison, 1978, 132.
145. Foster, 1876, v. On Foster see Geison, 1978.
146. Todd and Bowman, 1845, vol 1, 35 - 7.
147. J. Burdon Sanderson, 1879, 1 - 2.
148. Ibid, 13 - 5.
149. Ibid, 44 - 8.
150. Foster, 1877, 1.
151. Ibid, 299.
152. Ibid, 274ff.
153. Ibid, 321 - 2.
154. Ibid, 326.
155. See Kohler, 1973.
156. J. Burdon Sanderson, in G. Burdon Sanderson, 1911, 215 - 34 (220 - 5).
157. Bowditch, 1870 - 1, 142.
158. J. Burdon Sanderson, 1870 - 1, 189.
159. G. Burdon Sanderson, 1911, 75 - 80.
160. Geison, 1978, 81 - 115.
161. Anon, 1870 - 1, 74.
162. French, 1971, 30.
163. Stricker, 1870, 349 - 50, 369.
164. Commission, 1874, vol 2, 240 - 1.
165. Ibid.
166. Ibid, 241.
167. L. Huxley, 1900, vol 2, 310.
168. Foster, 1877, ix.
169. **Huxley Papers**, 42.36.
170. T.H. Huxley, 'The connections of the biological sciences with medicine' (1881), in idem, 1893 - 4, vol 3, 347 - 73 ( 354).
171. Ibid, 355 - 6.
172. Ibid, 372.
173. Ibid.
174. Ibid, 373.
175. See, for example, Fraser, 1868..
176. See Chapter One above.
177. Gregory, 1977.
178. T.H. Huxley, 'The present condition of organic nature' (1863), in idem, 1893 - 4, vol 2, 302 - 29 (317 - 8).
179. John Goodsir, 1868, vol 1, 185.
180. John Goodsir, 'On life and organization' (1856), in idem, 1868, vol 1, 286 - 322 (290, 297).

181. John Goodsir, 'On the dignity of the human body' (1862), in *idem*, 1868, vol 1, 207 - 85 (208).
182. *Ibid*, 269.
183. *Ibid*, 270.
184. *Ibid*, 285.
185. Henry Alleyne Nicholson (1844 - 99) studied medicine and natural science at Edinburgh 1862 - 7. He held a chair in biology at Durham and in natural history at St Andrews and Aberdeen.
186. Morley, 1917, vol 1, 90.
187. T.H. Huxley, 'On the physical basis of life' (1868), in *idem*, 1893 - 4, vol 1, 130 - 65 (130 - 1).
188. *Ibid*, 137 - 8.
189. *Ibid*, 152 - 3.
190. *Ibid*, 154.
191. *Ibid*, 156 - 62, 164.
192. J. Tyndall, 'Scientific materialism' (1868), in *idem*, 1879, vol 2, 75 - 90 (81).
193. *Ibid*, 82 - 3.
194. J. Tyndall, 'Scientific use of the imagination' (1870), in *idem*, 1879, vol 2, 101 - 36 (131 - 3).
195. *Ibid*, 133.
196. Joseph Goodsir, 1868a, 10 - 11.
197. *Ibid*, 17 - 8.
198. Stirling, 1869, 33.
199. *Ibid*, 38 - 9.
200. J. Young, 1869, 246.
201. *Ibid*. 250.
202. *Ibid*, 253 - 4.
203. *Ibid*, 263.
204. Quoted in Beale, 1871, 28.
205. Beale, 1861, 24 - 8, 178 - 82.
206. Beale, 1865, iii, 5, 199.
207. *Ibid*, 204 - 16.
208. Beale, 1874, 358.
209. *Ibid*.
210. Beale, 1899, 27.
211. Lewes, 1876, and 1877, 3 - 18.
212. See Chapter Six below.

## Chapter Four: Mind and Nature

1. Bell, 1966, 4. Charles Bell (1774 - 1842) was Professor of Surgery at Edinburgh. Besides the works referred to, he was the author of a 'Bridgewater Treatise' showing the perfect adaptation of structure to function in the hand. During the 1830s Bell was a prominent member of London medical society.
2. Ibid.
3. On the 'common sensorium' see Figlio, 1975.
4. Bell, 1966, 13.
5. Ibid, 5.
6. Ibid, 21 - 2.
7. On cerebral localisation see R.M. Young, 1970, 9 - 53 and 58 - 74; Clarke and O'Malley, 1968, 458 - 575. See also Flourens, 1842, and Cranefield, 1974.
8. Bell, 1836, 22 and Plate iv. The 'pyramidal body' was usually sub-divided into anterior and posterior columns and viewed as a direct continuation of the spinal marrow; together with the 'restiform bodies' the pyramids formed the 'medulla oblongata' which, with the 'pons variolii', was the link between the spine and the roots of the cerebrum.
9. Ibid, 22 - 3.
10. Bell, 1966, 35 - 6.
11. Bell, 1836, 209 - 11.
12. Ibid, 193.
13. Ibid, 224 - 5.
14. Fearing, 1964, esp. 68 - 96.
15. Unzer and Prochaska, 1851, 430 - 2.
16. Bichat, 1805, 79 - 80, 90 - 1. See also Albury, 1977, 75 - 82.
17. Gall and Spurzheim, 1810, vol 1, 80.
18. Liddell, 1960, 60 - 75. Marshall Hall (1790 - 1857) was a physician in Nottingham and later in London. He was a member of the 'progressive party' in the medical profession: a fact which does much to explain the hostility his ideas aroused in certain quarters. For his major publications see M. Hall, 1837; 1843; and 1850.
19. Hall, 1837, 5.
20. Ibid.
21. Ibid, 20.
22. See the account in the biography written by Hall's widow: C. Hall, 1861, 89 - 103.
23. M. Hall, 1837, 49 - 50.
24. Ibid, 70.
25. Hall, 1850, vii.
26. Ibid.
27. Serres, 1824, vol 1, 68.
28. Solly, 1836, xv. Samuel Solly (1805 - 71) studied in Paris in the late 1820s and took up an appointment as assistant surgeon at St Thomas's Hospital London in 1841; twelve years later he was appointed to a lectureship in surgery there.
29. See Ackerknecht, 1974.
30. Bichat, 1805, 325 - 31.
31. Ibid, 332.
32. Bichat, 1812, vol 1, 213, 218 - 9.
33. See, for instance, Béclard, 1821, 68.
34. Bell, 1836, 7 - 9.
35. Solly, 1836, 15.
36. Ibid, 15 - 6.
37. Ibid, 17.
38. Solly, 1847, 15. 9-42
39. J. Müller, 1838-40, vol 1, 607. Two English translations of Kölliker's **Handbook** of 1852 appeared during this period: see Kölliker, 1853 and 1860. The relevant section in the latter is 210 - 62, esp. 210 - 16.

40. See Todd, 1845.
41. See John Goodsir, 1845, 7, 25.
42. Solly, 1847, 17.
43. For this 'new' view of the nervous system see Todd and Bowman, 1845, vol 1, 265 - 7, 347; Solly, 1847, 331 - 3; and R. Todd, 'Nervous system', in idem, 1836 - 59, vol 3, 585 - 723, esp. 626 - 723.
44. See Todd, 1845, x - xiii.
45. Todd and Bowman, 1845, vol 1, 26 - 7, 234.
46. Ibid, 321.
47. Ibid, 328.
48. Ibid, 343, 347 - 50.
49. Ibid, 313.
50. An evolutionary perspective would seem to be implied here. Darwin's later stress on transformation by the accumulation of small variations and upon the significance of 'rudimentary' and non-functional structures would appear to confirm this assumption. But the provenance of these notions is more properly sought in the 'idealist' morphology of the early nineteenth century. As argued in Chapter Five, the similarities between such concepts and aspects of Darwin's thought need to be regarded not in terms of an 'anticipation' of evolutionary ideas but as part of Darwin's attempt to maximise the appeal of his theory by showing its application to the major preoccupations of contemporary biology.
51. Carpenter, 1846a, 439.
52. Ibid, 490 - 1.
53. Ibid, 504.
54. Ibid, 536.
55. Carpenter, 1853, x - xii.
56. Ibid, 672.
57. Carpenter, 1846a, 503; 3rd ed, 1856a, 592.
58. Carpenter, 1856a, 593 - 4.
59. Laycock, 1840, 85 - 6. Thomas Laycock (1812 - 76) was lecturer on clinical medicine at York Medical School until 1855 when he became Professor of the Practice of Medicine at Edinburgh.
60. Ibid, 96 - 7.
61. Ibid, 107.
62. Laycock, 1844, 16.
63. Laycock to Combe, 27.2.1845, **Combe Papers**, 7276 ff 3 - 4 (4).
64. Laycock, 1855, 155.
65. Laycock, 1833 - 57, 26 - 7, 65 - 6. Carmichael, 1830, 8 - 21.
66. On the British phrenology movement see Cooter, 1976 and 1978; Shapin, 1975; de Giustino, 1975.
67. Laycock to Combe, 26.3.1845, **Combe Papers**, 7276 ff 5 - 7.
68. Laycock, 1859, 556. Combe, for his part, was prepared to go a considerable way to accommodate phrenology to the principles of the new physiological psychology; for instance he concurred with Laycock that the grey matter of the hemispheres was the histological base of mind: Combe to Laycock, 31.3.1845, **Combe Papers**, 7290 ff 90 - 2. See also G. Combe, 1845, 162 - 6.
69. Laycock, 1844, 12 - 3.
70. Laycock to Combe, 9.6.1845, **Combe Papers**, 7276 ff 14 - 7 (15).
71. Laycock, 1855, 156.
72. K. Macleod, 1908, 11.
73. Laycock, 1855, 156.
74. Bain, 1855, 289 - 4; idem, 1859, 297 - 308. On Bain see Boring, 1929, 223 - 31 and R.M. Young, 1970, 101 - 33.
75. Bain, 1855, 296. Bain's later statements were more unequivocally hostile to consciousness as an active principle: see Bain, 1867, 374 - 5.
76. Gall and Spurzheim, 1810, vol 1, vii.
77. Blandford, 1871, 4 - 5.
78. Of the authors discussed below who argued for a somatic aetiology of insanity, the following were members of the Association of Medical Officers of Asylums and Hospitals for the Insane:

A. Addison, Assistant Physician, Royal Asylum Montrose;  
 J.T. Arlidge, St Lukes Hospital;  
 G.F. Blandford;  
 W.A.F. Browne, Crichton Institute and Commissioner in Lunacy for Scotland;  
 J.G. Davey, Medical Superintendent Hanwell and Colney Hatch Asylums;  
 T. Laycock, Honorary Member;  
 H. Maudsley, Superintendent County Asylum at Cheadle and of Manchester Royal Lunatic Hospital;  
 H. Monro, St Lukes Hospital;  
 D.H. Tuke, Visiting Physician York Retreat.

More difficult to illustrate is the **presumption** in favour of physicalist explanation of insanity which permeates the writings of British alienists of this period.

79. Morris, 1844, 6 - 7.
80. Monro, 1851, iii - vi.
81. Ibid, 12 - 3.
82. Bucknill and Tuke, 1862, 383 - 4.
83. Ibid, 385.
84. Addison, 1863, 37. This essay won a prize offered by the Superintendents of Asylums. A translation of Virchow's major work in this field appeared in 1860: Virchow, 1860. For a more general discussion of the reception of these ideas see W.H. McMenemy, 'Cellular pathology, with special reference to the influence of Virchow's teaching on medical thought and practice', in Poynter, 1968, 13 - 43; and Maulitz, 1978.
85. Bucknill and Tuke, 1862, 415.
86. Winslow, 1860, 19 - 20.
87. Addison, 1863, 44.
88. Bucknill and Tuke, 1862, 507 - 8.
89. Ibid, 510.
90. For a compilation of mental pathological and therapeutical writings from the earlier period see Hunter and Macalpine, 1963, esp. 234, 344, 370 - 2.
91. Bucknill and Tuke, 1862, 518, 530, 535 - 7.
92. Maudsley, 1867, 435 - 40. Henry Maudsley (1835 - 1918) was Medical Superintendent of the Manchester Royal Lunatic Hospital 1859 - 62, editor of the *Journal of Mental Science* 1862 - 70, and Professor of Medical Jurisprudence at London University 1869 - 79.
93. Browne to Combe, 22.5.1849 and 14.8.1857, *Combe Papers*, 7299 ff 39 - 40 (40) and 7539 ff 117 - 9 (118 - 9). Emphasis added.
94. Browne to Combe, 18.4.1857, *Combe Papers*, 7312 ff 148 - 9 (149). See also Browne to Combe, 30.4.1857, *ibid*, 7312 ff 150 - 3 (152).
95. Browne, 1864, 311.
96. Scull, 1975.
97. K. Jones, 1967, 15 - 6; Scull, 1979, 172 - 4. For a contemporary exposition of the alienists' grievances see Arlidge, 1859, 31 - 40, 101 - 3, 113.
98. See Bynum, 1964.
99. Arlidge, 1859, 104.
100. Scull, 1979, 159.
101. Lawrence, 1822, 99 - 100.
102. For a statement of the relevance of phrenology to the treatment of insanity see A. Combe, 1831, esp. 74 - 81. On the relation between phrenology and the British psychiatric profession see Cooter, 1976a.
103. Carpenter's attack on phrenology was seen as the beginning of the end for the scientific respectability of the creed: Carpenter, 1846, 751 - 7.
104. Browne to Combe, 30.4.1857, *Combe Papers*, 7321 ff 150 - 3 (151 - 2); see also *idem*, 18.4.1857, 7312 ff 148 - 9 (149).
105. Laycock, 1862, 3, 23.
106. Bucknill, 1857, 257 - 8. See also Bucknill and Tukes censures upon Mayo, 1854: Bucknill and

- Tuke, 1862, 498.
107. Bucknill and Tuke, 1862, 415 - 7.
  108. Ibid, 416 - 8. See also Davey, 1853, 1 - 6, 20 - 8, 43 - 9.
  109. Tuke, 1865, 261.
  110. R. Smith, unpublished.
  111. **Parliamentary debates**, 3rd series, 1862, 165, 1282 - 1305 (1292).
  112. See, for example, Tuke, 1865.
  113. Laycock, 1862a, 3. Best, 1971, 141.
  114. See Peterson, 1978, 194 - 243.
  115. See, for instance, the conjunction of a particular protest against the prevalence of legal definitions of madness and a general complaint against the low social status of the medical profession in Burgess, 1858, v - vii.
  116. Laycock, 1846, 4.
  117. Ibid, 13 - 4.
  118. Ibid, 4 - 5.
  119. Mapother, 1868, 1, 6.
  120. Ibid, 7 - 10.
  121. Ibid, 43 - 7, 67. See also R.M. Macleod, 'The anatomy of state medicine: concept and application', in Poynter, 1968, 199 - 227.
  122. Thomson, 1870, 491 - 2.
  123. Ibid, 488.
  124. Ibid, 492.
  125. Maudsley, 1873, 28.
  126. Thomson, 1870, 497 - 8.
  127. Maudsley, 1873, 20.
  128. Ibid, 23 - 4.
  129. Ibid, 33 - 5.
  130. Maudsley, 1879, 103 - 5. See also Browne, 1866, 310.
  131. MacKenzie, 1976, and 1980, Chap. 2.
  132. Pringle-Pattison, 1893, 555.
  133. Croom Robertson, 1876, 1 - 3. George Croom Robertson (1842 - 92) was a student of Bain. He was appointed Professor of Mental Philosophy and Logic at University College London in 1866 with the help of the Liberal faction; his major opponent was the clerical candidate, James Martineau.
  134. Lewes, 1879, vol 1, 3 - 5.
  135. Cf. G.H. Brown, 1955, 172 - 3.
  136. Maudsley, 1867, 40.
  137. Spencer, 1870, vol 1, 291.
  138. Ibid, 1st ed., 1855, 349.
  139. Ibid (1855), 349 - 51, 491 - 2, 534.
  140. Maudsley, 1867, 40. See also idem, 1883, 16 - 20, 128 - 48.
  141. Ibid, 38 - 9.
  142. Lewes, 1879, vol 1, 8, 21.
  143. Ibid, 14 - 7, 38.
  144. Spencer, 1870, 395 - 402; C.R. Darwin, 1901, 38 - 194.
  145. See Romanes, 1883 and 1888; C.L. Morgan, 1890 - 1. For discussions of the impact of Darwin upon psychology see Ghiselin, 1973; J.C. Greene, 1977; E.R. Hilgard, 'Psychology after Darwin', in Tax, 1960, vol 2, 269 - 85; Richards, 1977. For a broader view of the role of evolutionary notions in nineteenth century social psychology see Burrow, 1966.
  146. Engelhardt, 1975, 139. See also Greenblatt, 1977.
  147. Jackson, 1875, i - iii.
  148. Jackson, 1931, vol 1, 52.
  149. R.M. Young, 1970, 209. For later developments in neurology see W.H. Magoun, 'Evolutionary concepts of brain function following Darwin and Spencer', in Tax, 1960, vol 2, 187 - 209.
  150. C.R. Darwin, 1965, 66; see also 12 - 3, 29.

151. Ibid, 70 - 1, 81.
151. Ibid, 356.
153. Ibid, 365. On the naturalistic interest implicit in Darwin's comparative psychology see Durant, 1977, 151 - 3. See also C.U.M. Smith, 1978, 245 - 67.
154. Loeb, 1964. See also Gussin, 1963.
155. Croom Robertson, 1884, v.
156. Boring, 1929, 452 - 3.
157. Hamilton, 1859, vol 1, 264, 404 - 44.
158. Shapin, 1975, 226 - 31. See also Cooter, 1976a, 10 - 11.
159. Martineau, 1860, 500 - 1.
160. Ibid, 501.
161. Grote, 1865, x - xi. John Grote (1813 - 66) was an Anglican priest and fellow of Trinity College Cambridge. He was Knightsbridge Professor of Moral Philosophy from 1855.
162. On Sidgwick see Schneewind, 1977.
163. R. Smith, 1970, 42 - 4.
164. Quoted in Lewes, 1879, vol 2, 6.
165. See Chapter Two, sec. ii above.
166. Pringle-Pattison, 1893, 557 - 68.
167. Stout, 1891, 45; see also idem, 1931. For a more general discussion see Daston, 1978. George Frederick Stout (1860 - 1944) was a student of Ward. He lectured in Moral Sciences at Cambridge from 1894 - 6 and was Professor of Logic at St Andrews from 1903.
168. Boring, 1929, 456 - 7. James Ward (1843 - 1925) studied philosophy at Cambridge and physiology at Berlin and Göttingen. He was Professor of Moral Philosophy at Cambridge from 1897.
169. See Chap. Six, sec. ii.
170. Huxley to Tyndall, 25.9.1873, **Huxley Papers**, 8.158.
171. Tyndall to Huxley, 29.8.1875, *ibid*, 8.174.
172. Laycock, 1860, vol 1, 75 - 6.
173. T.H. Huxley, 'On Descartes' "Discourse touching the method of using one's reason rightly and of seeking scientific truth" ' (1870), in idem, 1893 - 4, vol 1, 166 - 98 (179).
174. Ibid, 184 - 5, 191.
175. Ibid, 195.
176. T.H. Huxley, 'On the hypothesis that animals are automata and its history' (1873), in idem, 1893 - 4, vol 1, 199 - 250 (203).
177. Ibid, 206 - 13.
178. Ibid, 219.
179. Ibid, 236 - 40, 244.
180. Ibid, 242 - 3, 249 - 50.
181. See Chap. One, sec v above.
182. This tradition is discussed in the Introduction.
183. Laycock, 1860, vol 1, 310 - 4.
184. Bucknill, 1856, 176 - 7.
185. Ibid, 178 - 9.
186. A.J. Balfour, 1895, 74 - 5.
187. Morley, 1879, vol 1, 176.
188. W.K. Clifford, 'Mind and body' (1874), in idem, 1879, vol 2, 31 - 70 (33 - 4).
189. Ibid, 46 - 7. See also Clifford, 1879a.
190. Clifford, 1879, vol 2, 50 - 3.
191. Ibid, 56 - 7.
192. Ibid, 60 - 1.
193. Ibid, 63 - 4. See also W.K. Clifford, 'On the nature of things-in-themselves' (1878), in idem, 1879, vol 2, 71 - 88.
194. Clifford, 1879, vol 2, 63, 87 - 8.
195. Ibid, 67 - 8.
196. Tyndall, 1878, 107 - 8.

197. Clifford, 1879, vol 2, 69 - 70.
198. Ibid, 58.



# Chapter Five: Evolution, Ontogeny, and the Natural System.

1. Charles Darwin to Richard Owen, Tuesday [1849], *Darwin-Lyell Correspondence*, BD25M.
2. Bell, 1852, 9.
3. On Geoffroy and Cuvier see Appel, 1975. On the type theory see Farber, 1976.
4. On these controversies see Ospovat, 1978, 35 - 9.
5. Lonsdale, 1870, 244 - 8.
6. Knox, 1843, 22.
7. Owen, 1848, 1 - 3. See also idem, 1866 - 8, vol 3, 786 - 8. Richard Owen (1804 - 92) was a friend of William Whewell and an associate of John Abernethy. Owen was Hunterian Professor of Comparative Anatomy 1836 - 56; thereafter he was superintendent of Natural History at the British Museum.
8. Owen, 1848, 7 - 8.
9. Ibid, 81.
10. Ibid, 106 - 7.
11. Owen, 1849, 171 - 2.
12. Ibid, 85 - 6.
13. See Brooke, 1977a, 143.
14. Owen 1849, 86. See also Yeo, 1977, 360 - 70.
15. T.H. Huxley, 'Owen's place in the history of the anatomical sciences', in Owen jr., 1894, vol 2, 273 - 332 (298 - 9).
16. von Baer, 1853, 183.
17. M. Barry, 1837, 116.
18. Ibid, 127 - 31.
19. Ibid, 136 - 7.
20. M. Barry, 1837a, 346.
21. Owen, 1843, 368 - 70.
22. See Ospovat, 1976, 17 - 24.
23. Owen, 1846, 174. Owen only became reconciled to the taxonomic importance of development much later: see Owen, 1883.
24. Owen, 1859.
25. Quoted in Winsor, 1976, 75.
26. T.H. Huxley, 'On the anatomy and the affinities of the family of the Medusae' (1849), in idem, 1898, vol 1, 9 - 32 (9).
27. Ibid, 10 - 23.
28. Ibid, 23.
29. Ibid, 23 - 5.
30. Ibid, 28.
31. T.H. Huxley, 'An account of researches into the anatomy of the hydrostatic Acephalae' (1851), in idem, 1898, vol 1, 98 - 101. See also Winsor, 1976, 77 - 80.
32. T.H. Huxley, 'On the morphology of the Cephalous Mollusca' (1853), in idem, 1898, vol 1, 152 - 93 (152 - 3).
33. Ibid, 170.
34. Ibid, 171 - 3.
35. Ibid, 177 - 90.
36. T.H. Huxley, 'Notes on classification', *Huxley Papers*, 40.149 - 50.
37. T.H. Huxley, 1898, vol 1, 176.
38. R.M. MacLeod, 1965, 262 - 3.
39. See Owen to Henri Milne-Edwards, 5.10.1856., *Owen Papers*, BOw 2.8.
40. Carpenter to Huxley, 22.10.1858., *Huxley Papers*, 12.94.
41. See Owen, 1855, 493. In describing the anal vent of the Brachiopod *Terebratula*, Owen had remarked: 'Mr Huxley has been unable to find this vent, and describes the anal end as imperforate. There may be blindness somewhere, but I think not at the termination of the intestine of *Terebratula*.'
42. Carpenter to Huxley 16.7.1855., *Huxley Papers*, 12.78.

43. T.H. Huxley in Owen jr., 1894, vol 2, 317 - 8.
44. Owen 1864, 4 - 5.
45. See Coleman, 1976, 152 - 3.
46. T.H. Huxley, 'Introduction' to von Baer, 1853, 176.
47. T.H. Huxley, 'On the theory of the vertebrate skull' (1859), in idem, 1898, vol 1, 538 - 606 (538).
48. Ibid, 539 - 40.
49. Ibid, 540.
50. Ibid, 540 - 1.
51. The petrosal is the bone at the side of the skull which contains the auditory labyrinth; the squamosal is a bone applied to the surface of the petrosal, parietal and alisphenoid.
52. Ibid, 550 - 1.
53. Ibid, 552 - 3.
54. For a more detailed representation of Huxley's views on the unity of plan in the vertebrate skull see T.H. Huxley, 1864.
55. T.H. Huxley, 1898, vol 1, 553 - 4.
56. For a discussion of the role of interests in moulding anatomical classification see S. Shapin, 'The politics of observation: cerebral anatomy and social interests in the Edinburgh phrenology disputes', in Wallis, 1979, 139 - 78.
57. The alisphenoids are bones attached to the superior portion of the basisphenoid in sheep and birds; they are adjacent to the petrosal.
58. T.H. Huxley, 1898, vol 1, 554 - 5.
59. Ibid, 558 - 9.
60. Ibid, 559 - 60.
61. Ibid, 556 - 7.
62. Ibid, 560.
63. Ibid, 560 - 1.
64. Ibid, 570. For the argument about the carp presphenoid see 559.
65. Ibid.
66. Ibid, 570 - 1.
67. Ibid, 571.
68. Ibid, 571 - 4.
69. Ibid, 578.
70. Ibid, 582 - 2.
71. Ibid, 584 - 5.
72. See Bartholemew, 1975.
73. Lyell to Huxley, 17.6.1859., *Huxley Papers*, 6.20.
74. Huxley to Lyell, 26.6.1859., *Darwin-Lyell Correspondence*, BD25L.
75. Quoted in J. Oppenheimer, 'An embryological enigma in the *Origin of Species*', in idem, 1967, 221 - 55 (221 - 2).
76. Darwin to Huxley, 23.4. 1853, *Huxley Papers*, 5.4.
77. T.H. Huxley, 1898, vol 1, 190 - 2.
78. T.H. Huxley, 'Notes on classification', *Huxley Papers*, 40.150 - 1.
79. Darwin to Huxley, 23.4.1853, *ibid*, 5.4.
80. T.H. Huxley, 'On the common plan of the animal form' (1854), in idem, 1898, vol 1, 281 - 3.
81. Darwin to Huxley, 8.3.1855., *Huxley Papers*, 5.25.
82. Darwin to Huxley, 3.10, 1857., *ibid*, 5.139.
83. Darwin to Huxley, 26.9.1857., *ibid*, 5.54.
84. C.R. Darwin, 1958, 125.
85. C.R. Darwin, 1901, 397 - 8. See also the diagram on 160 - 1.
86. Ibid, 398 - 9.
87. Ibid, 399 - 400.
88. Ibid, 403 - 4, 415.
89. Ibid, 415 - 6.
90. Ibid, 417 - 9.
91. Ibid, 419 - 22.

92. Ibid, 422 - 6.
93. Ibid, 427 - 8.
94. C. R. Darwin, 'Prefatory note', to Weismann, 1882, vol 1, v - vi, (vi).
95. Russell, 1916, 247.
96. Gegenbaur, 1859; idem, 1878.
97. Coleman, 1976, 150 and 162.
98. F. Müller, 1869, 111.
99. Ibid, 112.
100. Weismann, 1882, vol 1, 270.
101. Russell, 1916, 312 - 3. For a detailed discussion of the recapitulation theory see Gould, 1977, 69 - 114 (esp. 76 - 85). For a more general account of the reception of Darwinism in Germany see W. Montgomery, 'Germany', in Glick, 1972, 81 - 116 (esp. 105 - 10).
102. T.H. Huxley, 'On the classification of the animal kingdom' (1876), in idem 1898, vol 4, 35 - 60 (35 - 7).
103. See, for example, T.H. Huxley, 'Palaentology and the doctrine of evolution' (1870), in idem, 1893 - 4, vol 8, 272 - 304.
104. De Beer, 1937, 14.
105. Parker and Bettany, 1877, v - vi.
106. Parker, 1871, 200 - 1.
107. Parker, 1869, 807 - 8.
108. Parker and Bettany, 1877, 310, 357 - 8.
109. Lankester, 1870, 34 - 5.
110. Ibid.
111. Ibid, 36 - 9.
112. Lankester, 1873, 321 - 3.
113. Ibid, 323.
114. Ibid, 324 - 5.
115. Ibid, 327 - 8.
116. Ibid, 329 - 34.
117. Ibid, 335 - 7.
118. Lankester, 1877, 400 - 2.
119. Ibid, 403 - 5.
120. Ibid, 405 - 7.
121. Ibid, 416 - 7.
122. Ibid, 422 - 3.
123. Ibid, 436 - 40.
124. Ibid, 432 - 3.
125. Ibid, 434.
126. Lankester, 1876, 51 - 2.
127. Lankester, 1876 - 7, 65 - 6.
128. See, for example, F.M. Balfour, 1875 and 1880.
129. Geison, 1978.
130. Ibid, 127 - 8.
131. Foster and Balfour, 1874.
132. F.M. Balfour, 1885.
133. A.H.G., 1874, 127.
134. Poulton, 1886 - 7, 601. Haddon, 1887.
135. Lankester, 1887 - 8, 506 - 7.
136. Lankester, 1881, 25.
137. F.M. Balfour, 1885, vol 1, 3.
138. Ibid, 4 - 6.
139. Cf. Russell, 1916, 268 - 87.
140. F.M. Balfour, 1885, vol 1, 502 - 3.
141. Ibid, 504 - 9.
142. Ibid, vol 2, 311 - 8.

143. Ibid, 327.
144. Ibid, 360 - 9.
145. Ibid, 363 - 4, 384.
146. Lubbock, 1874, esp. 28 - 9, 34 - 40, 62 - 6.
147. Ibid, 87 - 93.
148. A.M. Marshall, 'The recapitulation theory' (1890), in idem, 1897, 289 - 363 (289 - 92).
149. A.M. Marshall, 1893, 25 - 6.
150. D.N.B.
151. Sedwick, 1894, 40 - 1.
152. For the agitation see Anon, 1884.
153. On Roux and **Entwicklungsmechanik** see T.H. Morgan, 1898; and F.B. Churchill, 'Chabry, Roux, and the experimental method in nineteenth-century embryology', in Giere and Westfall, 1973, 161 - 205.
154. His, 1888, 291 - 5.
155. D'Arcy Thompson, 1917, 56 - 7.
156. Needham, 1931, vol 1, 1 - 4, 9.

# Chapter Six: Naturalism and Society, 1880 - 1914.

1. Peel, 1977, 228. See also Perkin, 1978, esp. 252 - 70; and Reader, 1966, 146ff.
2. Spencer, 1896, 548.
3. Spencer, 1895, 545 - 6.
4. H. Perkin, 'Land reform and class conflict in Victorian Britain', in Butt and Clarke, 1973, 177 - 217 (202).
5. Chapter One. sec. v.
6. Kent, 1899, 406 - 9.
7. Kebbel, 1892, 8.
8. Macobby, 1938, 150.
9. Ibid, 148 - 82, 187 - 8.
10. Kent, 1899, 171.
11. Mallock, 1884, 4 - 5. William Hurrell Mallock (1849 - 1923) was a writer of religious and social works. He was closely involved with the Conservative Party after 1880.
12. Ibid, 28.
13. W.K. Clifford, 'Mind and body' (1874), in idem, 1879, vol 2, 31 - 70 (59).
14. Quoted in Mallock, 1884, 57.
15. Ibid, 57 - 66.
16. Lilly, 1889, 77 - 8, 10. William Samuel Lilly (1840 - 1919) was Secretary to the Catholic Union of Great Britain. He wrote extensively on the claims of Christianity and on contemporary political issues.  
Mallock, 1889, 249, 256.
17. See Chapter Two, sec. v.
18. A.J. Balfour, 1879, chaps. 2 - 3. For a fuller account see Jacyna, 1980, 11 - 19.
19. A. Seth Pringle-Pattison, 'Notes on Lord Balfour's philosophy', in Dugdale, 1936, vol 2, 413 - 34 (428). Andrew Seth Pringle-Pattison (1856 - 1931) was a Scottish philosopher who successively held chairs at Cardiff, St Andrews, and Edinburgh. He attempted to reconcile Hegelian and Christian notions in his philosophy and also exploited the resources of native Scottish philosophy to controvert empiricism and agnosticism.
20. A.J. Balfour, 1930, 27 - 8.
21. Curtis, 1963, 406.
22. Jennings, 1881, 300.
23. Perkin, 1978, 452 - 4.
24. See Cole, 1954, 379 - 424.
25. Macobby, 1938, 323 - 41; idem, 1958, 83 - 90; Edwards, 1894, 269.
26. Mivart, 1885, 440. St George Jackson Mivart (1827 - 1900) was a biologist and a Christian apologist. He was one of the leading critics of Darwinism and of Huxley's naturalistic interpretation of evolution.
27. Mallock, 1883, 353 - 4.
28. Perkin, in Butt and Clarke, 1973, 210.
29. Haldane, 1888, 145.
30. Mallock, 1884a, 93.
31. de Laveleye, 1883, 576.
32. Roach, 1957, 80, 58. See also Harvie, 1976, chap. 9.
33. See the case studies in Turner, 1974a.
34. Bosanquet, 1906, 1.
35. A. Seth Pringle-Pattison, 'The present position of the philosophical sciences' (1891), idem, 1897, 34 - 63 (57 - 60).
36. Ward, 1899, vol 1, x.
37. J. Ward, 'The present trend of philosophical speculation' (1903), idem, 1927, 162 - 81 (167).
38. Manning, 1883, 20 - 3.
39. Kaufmann, 1885, 823 - 4, 839.
40. W.F. Barry, 1891, 67, 92 - 3.
41. Lilly, 1890, 18 - 24, 150 - 1.

42. Sidgwick, 1893, 503 - 6.
43. A.J. Balfour, 'The religion of humanity' (1888), *idem*, 1893, 283 - 344 (291 - 3).
44. *Ibid*, 313 - 4.
45. A.J. Balfour, 1895, 14, 18 - 9.
46. *Ibid*, 86.
47. A. Seth Pringle-Pattison, 'The legitimacy of the argument from consequences' (1896), *idem*, 1897, 304 - 8 (306 - 7).
48. Mivart, 1895, 263, 266.
49. Balfour to Pringle-Pattison, July 1886, **Balfour Papers**, Add MSS 49798, 7 - 33 (18).
50. A.J. Balfour, 1895, 301, 321.
51. A.J. Balfour, 1904, 5, 24.
52. A.J. Balfour, 1895, 194ff.
53. *Ibid*, 228 - 9.
54. Pringle-Pattison, in Dugdale, 1936, vol 2, 428.
55. Pringle-Pattison, 1917, 62 - 3.
56. K. Mannheim, 'Conservative thought', in *idem*, 1953, 74 - 164 (79).
57. *Ibid*, 89 - 90, 96 - 7.
58. Searle, 1971, 31.
59. Rothblatt, 1968, esp. 119 - 38, 143 - 4.
60. Martineau, 1886, 6 - 8.
61. Pringle-Pattison to Blackie, 16.1.1882, **Pringle-Pattison Papers**, 2634 ff 131.
62. Pringle-Pattison, 1917, 73.
63. Parsons, 1900, 185 - 7. Harold George Parsons (d.1905) was a journalist and soldier. He was a spokesman of Conservative Imperialists at the turn of the century.
64. *Ibid*, 188 - 9;
65. Lilly, 1889, 190 - 1.
66. Crackanthorpe, 1908, 972.
67. Macobby, 1938, 329.
68. Graham, 1895, 23. See also Shannon, 1976, 235.
69. D.J. Ford, 'W.H. Mallock and socialism in England, 1880 - 1918', in K.D. Brown, 1974, 317 - 42 (318 - 9).
70. Helfand, 1977; Bannister, 1979, chap. 7. My discussion of Huxley's thought in the 1880s and 1890s is an elaboration of Helfand's thesis.
71. See Huxley to Tyndall, 9.11.1866, **Huxley Papers**, 8.52.
72. Tyndall to Huxley, 19.11.1868, *ibid*, 8.67.
73. Mivart to Huxley, 13.4.1886, *ibid*, 22.282.
74. Tyndall to Huxley, 27.12.1887, *ibid*, 8.260.
75. Huxley to Tyndall, 1.1.1888, *ibid*, 8.262; Tyndall to Huxley, 4.1.1888, *ibid*, 8.263.
76. T.H. Huxley, 'A liberal education and where to find it' (1868), in *idem*, 1893 - 4, vol 3, 76 - 110 (82 - 5).
77. *Ibid*, 88 - 9.
78. T.H. Huxley, 'Science and culture' (1880), in *idem*, 1893 - 4, vol 3, 134 - 59 (150).
79. R. Smith, 1972, 197. See also Durant, 1979.
80. R. Smith, 1972, 181.
81. *Ibid*, 190ff.
82. T.H. Huxley, 'The struggle for existence in human society' (1888), in *idem*, 1893 - 4, vol 9, 195 - 236.
83. *Ibid*, 195 - 6.
84. *Ibid*, 197.
85. *Ibid*, 199.
86. *Ibid*.
87. *Ibid*, 201 - 2.
88. *Ibid*, 202 - 3.
89. *Ibid*, 203, 205.
90. *Ibid*, 205.

91. Ibid, 205 - 7.
92. Ibid, 207 - 8.
93. Ibid, 209.
94. Ibid, 210.
95. Ibid, 211 - 2.
96. Ibid, 213 - 8.
97. T.H. Huxley, 'Capital - the mother of labour: an economical problem discussed from a physiological point of view' (1890), in idem, 1893 - 4, vol 9, 147 - 87 (186 - 7).
98. Ibid, 150.
99. Ibid, 161 - 2.
100. Ibid, 163 - 5.
101. T.H. Huxley, 'On the natural inequality of men' and 'Natural rights and political rights' (1890), in idem, 1893 - 4, vol 1, 290 - 335 and 336 - 82.
102. 'On the natural inequality of men', 294.
103. Ibid, 295 - 6.
104. Ibid, 302.
105. Ibid, 303.
106. Ibid, 307.
107. Ibid, 308.
108. Ibid, 308 - 9.
109. Ibid, 312, 335.
110. 'Natural rights and political rights', 337 - 8.
111. Ibid, 339.
112. Ibid, 346.
113. Ibid, 351 - 2.
114. T.H. Huxley, 'Prolegomena', idem, 1893 - 4, vol 9, 1 - 45 (27).
115. T.H. Huxley, 'Government: anarchy or regimentation', in idem, 1893 - 4, vol 1, 383 - 430 (424).
116. Ibid, 424 - 7.
117. 'Natural rights', 362.
118. 'Government', 428.
119. Ibid, 430.
120. Mallock, 1883, 355.
121. Mallock, 1881, 934, 941. To some extent, 'conservative' naturalism was countered by 'progressive' naturalism during this period. But the latter was a less pervasive phenomenon. See, Freedon, 1976 and 1978, chap. 3.
122. Mallock, 1881, 942 - 3.
123. Mallock, 1895 (Part One), 905; ibid (Part Three), 240 - 3.
124. A.J. Balfour, 'A fragment on progress', idem, 1893, 241 - 82 (247 - 8).
125. Ibid, 251 - 3.
126. Parsons, 1900, 185.
127. Hobhouse, 1904, 65 - 9.
128. See Cecil, 1912, 118.
129. Zebel, 1973, 122; Emy, 1973, 124 - 5, 189 - 203.
130. See Halévy, 1952, vol 2, 449 - 90; Dangerfield, 1966, esp. 178 - 266; Meacham, 1972; and Emy, 1973, 105 - 18.
131. **Parliamentary debates**, 4th series, 1906, 160, 1392 - 5 (1394).
132. See Searle, 1976; MacKenzie, 1976.
133. Crackanthorpe, 1908, 971 - 2.
134. Searle, 1978.
135. See K.D. Brown, 'The Anti-Socialist Union', in idem, 1974, 234 - 61.
136. Eugenics, 1913, vol 2, 7 - 11 (8).
137. MacKenzie, 1979.
138. Freedon, 1979, 645 - 7.
139. Ibid, 647 - 8, 654.
140. K. Young, 1963, 255.

141. Peel, 1971, 228.

142. Freeden, 1979, 653.